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TRANSACTIONS

OF THE

AMERICAN

PHILOSOPHICAL SOCIETY,

HELD AT

PHILADELPHIA,

FOR PROMOTING

USEFUL KNOWLEDGE.

VOLUME II.

PHILADELPHIA:

PRINTED AND SOLD BY *ROBERT AITKEN*, AT POPE's HEAD IN MARKET STREET.

M.DCC.LXXXVI.

X ADAMS 103.6 a
v.2

Other ed. xx E. 145.2.2

ADVERTISEMENT.

THE peculiar circumſtances of America, ſince the publication of the firſt volume of the Tranſactions of this Society, will be a ſufficient apology for the long delay in publiſhing a ſecond. The Society having, however, reſumed their former labours in promoting uſeful knowledge, which were neceſſarily ſuſpended during the late war: and finding themſelves in poſſeſſion of materials more than ſufficient for a ſecond volume of Tranſactions, appointed a Committee to ſelect ſuch pieces as might be moſt proper for that purpoſe: The Committee have made that ſelection, which is here offered to the public. Several pieces ſtill remain worthy of publication, which will probably appear in a future volume.

It may not be amiſs in this place to inſert the Rules which the Society have adopted for the direction of their Committees in the choice of papers for publication.

FIRST, "That the grounds of the Com-
"mittee's choice of papers for the preſs, ſhould
"always be the importance or ſingularity of
"the ſubjects, or the advantageous manner of
"treating them, without pretending to an-
"ſwer, or to make the ſociety anſwerable,
"for the certainty of the facts, or propriety
"of the reaſonings, contained in the ſeveral
"papers ſo publiſhed, which muſt ſtill reſt on
"the credit or judgment of their reſpective
"authors.

SECONDLY, "That neither the Society, nor
"the Committee of the preſs, do ever give
"their opinion as a body, upon any paper
"they may publiſh, or upon any ſubject of
"Art or Nature that comes before them."

LAWS

LAWS AND REGULATIONS,

OF THE

American PHILOSOPHICAL SOCIETY

HELD AT *PHILADELPHIA*, FOR PROMOTING

USEFUL KNOWLEDGE.

TWO ſocieties having formerly ſubſiſted in Philadelphia, whoſe views and ends were the ſame, viz. "*the advancement of uſeful knowledge,*" it was judged that their union would be of public advantage; and they were accordingly united *January* 2d, 1769, by a certain *Fundamental Agreement*; the chief *Articles* of which are,

Firſt, That the name of the *United Society* ſhall be *The American Philoſophical Society, held at Philadelphia, for promoting uſeful Knowledge.*

Secondly, That there ſhall be the following officers of the ſociety, viz. one *Patron*, one *Preſident*, three *Vice-Preſidents*, one *Treaſurer*, four *Secretaries*, and three *Curators.*

Thirdly, That all the above officers ſhall be choſen annually by ballot, at the firſt meeting of the Society in January; excepting only that inſtead of electing a Patron, the Governor of the Province be requeſted to be Patron.

Other Laws were to be made by the *United Society;* and accordingly the following LAWS, &c. were paſſed *February* 3d, 1769.

I. *Of the annual Payments to be made by Members.*

Every Member of this Society ſhall ſubſcribe *Ten Shillings* yearly, to be applied by the Society to ſuch purpo-

ſes

ſes as they ſhall direct; and no Member ſhall be intitled to a vote in the annual election of officers, unleſs it appears that he has paid into the hands of the Treaſurer, the ſubſcription of the preceding year, and all former arrears, if any there were.

Every Member hereafter to be choſen, agreeable to the Laws of this Society, ſhall pay *Ten Shillings* admiſſion money, and alſo ſubſcribe for the yearly payment of *Ten Shillings*, before he is intitled to have any vote in the buſineſs of the Society at their meetings.

II. *Of the Election of Members.*

The election of new Members ſhall be by ballot, and that only on the third Friday in the months of January, April, July and October; and in order to ſuch election at leaſt twenty Members muſt be preſent.

Any Member may, at any meeting, propoſe ſuch perſon or perſons, as he thinks proper to be a Member or Members of this Society; but no perſon ſhall be ballotted for, unleſs his name, together with the name or names of the Member or Members who propoſed him, has been fixed up by the Secretaries for the view of the Society, at the two meetings preceding the time of election. Nor ſhall any perſon be deemed duly choſen unleſs three-fourths of the votes of the whole Members be in his favour.

III. *Of the Officers, and manner of their election.*

The election of ſuch Officers as are to be choſen in this Society, ſhall be on the firſt Friday in January every year, by ballot or written ticket, between the hours of two and five in the afternoon, at ſuch place in this city as ſhall be fixed by the Society at their previous meeting on the third Friday in December every year; of which notice ſhall be given in the Gazette, or ſuch other public papers as the Society

Society ſhall order, at leaſt one week before the day of election.

Before opening the election, the company that ſhall be met at half an hour after two, ſhall appoint three Members of the Society as judges of the election, and alſo two clerks or ſecretaries, for taking down the names of the voters. And in caſe of an equality of votes for any Officer, after caſting up the ballots, the deciſion ſhall be by lots, to be drawn by one of the judges.

IV. *Of the Preſident.*

The Preſident is to preſide at all meetings, to preſerve order, to regulate the debates, and to ſtate and put queſtions, agreeable to the ſenſe and intention of the Members.

V. *Of the Vice-Preſidents.*

In the abſence of the *Preſident*, his duty ſhall devolve on the *Vice-Preſidents*, ſo that they ſhall preſide alternately at meetings. But if the Vice-Preſident, whoſe turn it is to preſide at any meeting, ſhould be abſent, his place ſhall be ſupplied by any of the other Vice-Preſidents, who ſhall be preſent, according as he may be next in turn. If only one Vice-Preſident be preſent, he ſhall of courſe preſide; and if neither the Preſident, nor any Vice-Preſident be preſent, the Members met, ſhall appoint one of their number to take the chair for that meeting.

VI. *Of the Treaſurer.*

The *Treaſurer* ſhall receive the ſubſcriptions of the Members, and all other monies that may become due to the Society, and ſhall pay the ſame agreeable to their orders, certified to him by the Preſident, Vice-Preſident or Member, who was in the chair when the order was made.

The

The Treaſurer ſhall keep a regular account of all monies received and paid by him as aforeſaid; and once every year, or oftener if required by the Society, he ſhall render an account to them of the ſtock in his hands, and the diſburſements made by their order, and ſhall deliver up to his ſucceſſor the books and all papers belonging to them, together with the balance of caſh in his hands. And for the faithful diſcharge of his truſt, he ſhall, before he enters on his office, give bond and ſecurity to the Preſident and Vice-Preſidents, in double the ſum which they, or any three of them, ſhall judge he may probably become entruſted with during his ſaid office.

VII. *Of the Secretaries.*

The *Secretaries* ſhall ſo ſettle matters as to take equal ſhares of all buſineſs, and ſo as that two of them ſhall ſerve at every meeting, viz. one to take the minutes, and one to read all letters and papers that may be communicated to the Society. It is alſo the buſineſs of the two Secretaries of each particular meeting, to copy into the minute-book the proceedings of that meeting, in order to produce the ſame fair to the next meeting. They are further to copy into the proper books all ſuch letters, papers and eſſays, as the Society may think fit to preſerve on record, and to have the ſame ready to be laid before the next meeting.

The other two Secretaries are, in the mean while, to give notice to new members of their election, and agreeable to the directions of the Society, to write or anſwer letters; and, in general, to *manage all matters of correſpondence.*

The Secretaries may, for their own eaſe, change places; ſo that the two who have ſerved as *correſponding Secretaries*, for one month or limited time, ſhall take their turn to ſerve for the like time as *ſitting or attending Secretaries.*

VIII. *Of*

VIII. *Of the Curators.*

The buſineſs of the *Curators* ſhall be to take charge of, and preſerve, all *Specimens of natural Productions*, whether of the *Animal*, *Vegetable* or *Foſſil* kingdom; all models of machines and inſtruments, and all other matters and things belonging to the Society, which ſhall be committed to them; to claſs and arrange them in their proper order, and keep an exact liſt of them, with the names of the reſpective donors, in a book provided for that purpoſe; which book ſhall be laid before the Society, as often as called for.

The Curators, on entering upon their office, ſhall give ſuch a receipt for every thing that is committed to their charge, as the Society ſhall think proper; and, at the end of their term, ſhall deliver up the ſame to their ſucceſſors. For the faithful performance of their duty, and of the truſt repoſed in them, they ſhall give bond to the Preſidents and Vice-Preſidents, in ſuch a ſum as they, or any three of them, ſhall require.

IX. *Of the Meetings of the Society.*

The ordinary meetings of the Society ſhall be on the firſt and third Fridays of every month, from October to May, both incluſive, at ſix o'clock in the evening, and on the third Friday in each of the other four months at ſeven o'clock.

No meeting ſhall be continued after ten o'clock, nor any new matter be introduced by motion, or otherwiſe, after nine o'clock.

X. *Of the Diſpoſition of Money, and making new Laws.*

No part of the Society's ſtock ſhall be diſpoſed of in *Premiums*, or otherwiſe, nor ſhall any new laws be made,

until the ſame have been propoſed at one meeting, and are agreed to by two-thirds of twenty or more Members preſent at a ſubſequent meeting.

XI. *Of other Proceedings of the Society.*

No queſtion ſhall be put on a *motion*, unleſs the motion be *ſeconded;* and the determination of any queſtion ſhall be by *ballot*, inſtead of open ſuffrage, if deſired by any four Members. In caſe of an equality of votes on any queſtion, the ſame ſhall be deferred to another meeting.

When any Member ſpeaks he ſhall ſtand up, and addreſs himſelf to the chair, and the reſt ſhall remain ſilent in their ſeats. When two or more offer to ſpeak at the ſame time, the preſiding Member, in that, as in other matters of order, ſhall regulate and determine who ſhall ſpeak firſt.

XII. *Of Committees.*

The Members of this Society ſhall be claſſed into one or more of the following *Committees.*

1. Geography, Mathematics, Natural Philoſophy and Aſtronomy.
2. Medicine and Anatomy.
3. Natural Hiſtory and Chemiſtry.
4. Trade and Commerce.
5. Mechanics and Architecture.
6. Huſbandry and American Improvements.

Theſe *Committees* ſhall meet on their own adjournments, and at ſuch other times as the Society ſhall appoint, for the conſideration of any matters referred to them, and ſhall have power to chuſe their own chairman. But no Committee, as ſuch, ſhall take up any new buſineſs of the Society, but ſhall confine themſelves only to the ſubjects for which they are appointed, and to matters referred to them by the Society.

An ACT for Incorporating the American Philoſophical Society, held at Philadelphia, for Promoting uſeful Knowledge.

WHEREAS the cultivation of uſeful knowledge, and the advancement of the liberal arts and ſciences in any country, have the moſt direct tendency towards the improvement of agriculture, the enlargement of trade, the eaſe and comfort of life, the ornament of ſociety, and the increaſe and happineſs of mankind. *And whereas* this country of North-America, which the goodneſs of Providence hath given us to inherit, from the vaſtneſs of its extent, the variety of its climate, the fertility of its ſoil, the yet unexplored treaſures of its bowels, the multitude of its rivers, lakes, bays, inlets, and other conveniencies of navigation, offers to theſe United States one of the richeſt ſubjects of cultivation, ever preſented to any people upon earth. *And whereas* the experience of ages ſhews that improvements of a public nature, are beſt carried on by ſocieties of liberal and ingenious men, uniting their labours, without regard to nation, ſect or party, in one grand purſuit, alike intereſting to all, whereby mutual prejudices are worn off, a humane and philoſophical ſpirit is cheriſhed, and youth are ſtimulated to a laudable diligence and emulation in the purſuit of wiſdom. *And whereas*, upon theſe principles, divers public ſpirited gentlemen in Pennſylvania, and other American ſtates, did heretofore unite themſelves, under certain regulations, into one voluntary ſociety, by the name of "*The Ameri-*" *can Philoſophical Society, held at Philadelphia for pro-*" *moting uſeful Knowledge*," and by their ſucceſsful la-

bours and inveſtigations, to the great credit of America, have extended their reputation ſo far, that men of the firſt eminence in the republic of letters in the moſt civilized nations in Europe, have done honour to their publications, and deſired to be enrolled among their members. *And whereas* the Society, after having been long interrupted in their laudable purſuits by the calamities of war, and the diſtreſſes of our country, have found means to revive their deſign, in hopes of being able to proſecute the ſame with their former ſucceſs, and being further encouraged therein by the public, for which purpoſe they have prayed us, *The Repreſentatives of the Freemen of the Commonwealth of Pennſylvania*, that they may be created one body politic and corporate forever, with ſuch powers, privileges, and immunities, as may be neceſſary for anſwering the valuable purpoſes which the ſaid Society had originally in view.

Wherefore, in order to encourage the ſaid Society in the proſecution and advancement of all uſeful branches of knowledge, for the benefit of their country, and of mankind; *Be it enacted, and it is hereby enacted, by the Repreſentatives of the Freemen of the Commonwealth of Pennſylvania in General Aſſembly met, and by the authority of the ſame*, That the members of the ſaid American Philoſophical Society heretofore voluntarily aſſociated for promoting uſeful knowledge, and ſuch other perſons as have been duly elected members and officers of the ſame agreeably to the fundamental laws and regulations of the ſaid Society, compriſed in twelve ſections, prefixed to their volume of Tranſactions, publiſhed in Philadelphia by *William* and *Thomas Bradford*, in the year of our Lord one thouſand ſeven hundred and ſeventy-one; and who ſhall in all reſpects conform themſelves to the ſaid laws and regulations, and ſuch other laws, regulations and ordinances, as ſhall hereafter be duly made and enacted by the ſaid Society, according to the tenor hereof, be, and forever

ever hereafter ſhall be one body corporate and politic in deed, by the name and ſtyle of *The American Philoſophical Society, held at Philadelphia, for promoting uſeful knowledge*, and by the ſame name they are hereby conſtituted and confirmed one body corporate and politic, to have perpetual ſucceſſion, and by the ſame name they and their ſucceſſors are hereby declared and made able and capable in law, to have, hold, receive, and enjoy lands, tenements, rents, franchiſes, hereditaments, gifts and bequeſts of what nature ſoever, in fee-ſimple, or for term of life, lives, years, or otherwiſe, and alſo to give, grant, let, ſell, alien, or aſſign the ſame lands, tenements, hereditaments, goods, chattels, and premiſes, according to the nature of the reſpective gifts, grants and bequeſts, made to them the ſaid Society, and of their eſtate therein.

Provided, That the amount of the clear yearly value of ſuch real eſtate do not exceed the value of ten thouſand buſhels of good merchantable wheat.

And be it further enacted by the authority aforeſaid, That the ſaid Society be, and ſhall be for-ever hereafter able and capable in law to ſue, and be ſued, plead and be impleaded, anſwer and be anſwered unto, defend and be defended, in all or any of the courts or other places, and before any judges, juſtices, and other perſon and perſons, in all manner of actions, ſuits, complaints, pleas, cauſes and matters, of what nature or kind ſoever, within this commonwealth; and that it ſhall and may be lawful to and for the ſaid Society, for-ever hereafter to have and uſe one common ſeal in their affairs, and the ſame at their will and pleaſure to break, change, alter and renew.

And be it further enacted by the authority aforeſaid, That for the well governing the ſaid Society, and ordering their affairs, they ſhall have the following officers, that is to ſay, one Patron, who ſhall be his Excellency the Preſident of the Supreme Executive Council of this commonwealth, for the time being, and likewiſe one Preſident,

ſident, three Vice-Preſidents, four Secretaries, three Curators, one Treaſurer, together with a Council of twelve Members; and that on the firſt Friday of January next, between the hours of two and five in the afternoon, as many of the members of the ſaid Society as ſhall have paid up their arrears due to the Society, and ſhall declare their willingneſs to conform to the laws, regulations and ordinances of the Society, then duly in force, according to the tenor hereof, by ſubſcribing the ſame, and who ſhall attend in the hall, or place of meeting of the ſaid Society, within the time aforeſaid, ſhall chooſe by ballot, agreeably to the fundamental laws and regulations herein before referred to, one Preſident, four Secretaries, three Curators, and one Treaſurer, and at the ſame time and place, the Members met and qualified as aforeſaid, ſhall in like manner chooſe four Members for the Council, to hold their offices for one year, four more Members for the Council to hold their offices for two years, and four more Members for the Council to hold their offices for three years. And on the firſt Friday in January, which ſhall be in the year of our Lord one thouſand ſeven hundred and eighty-two, and ſo likewiſe on the firſt Friday of January, yearly and every year thereafter, between the hours of two and five in the afternoon, the Members of the ſaid Society met and qualified as aforeſaid, ſhall chooſe one Preſident, three Vice-Preſidents, four Secretaries, three Curators and one Treaſurer, to hold their reſpective offices for one year; and four Councilmen, to hold their offices for three years. *Provided*, That no perſon reſiding within the United States ſhall be capable of being Preſident, Vice-Preſident, Secretary, Treaſurer, or Member of the Council, or of electing to any of the ſaid offices, who is not capable of electing and being elected to civil offices within the ſtate in which he reſides. *Provided alſo*, That nothing herein contained, ſhall be conſidered as intended to exclude any of the ſaid officers or counſellors, whoſe times ſhall be

expired, from being re-elected, according to the pleaſure of the ſaid Society; and of the day, hours, and place of all ſuch elections, due notice ſhall be given by the Secretaries, or ſome one of them, in one or more of the public news-papers of this ſtate, agreeably to the ſaid fundamental laws and regulations before referred to.

And be it further enacted by the authority aforeſaid, That the officers and council of the ſaid Society ſhall be capable of exerciſing ſuch power for the well governing and ordering the affairs of the Society, and of holding ſuch occaſional meetings for that purpoſe, as ſhall be deſcribed, fixed, and determined by the ſtatutes, laws, regulations and ordinances of the ſaid Society, hereafter to be made. *Provided always*, That no ſtatute, law, regulation or ordinance ſhall ever be made or paſſed by the ſaid Society, or be binding upon the Members thereof, or any of them, unleſs the ſame hath been duly propoſed, and fairly drawn up in writing, at one ſtated meeting of the Society, and enacted or paſſed at a ſubſequent meeting at leaſt the ſpace of fourteen days after the former meeting, and upon due notice in ſome of the public news-papers, that the enacting of ſtatutes and laws, or the making and paſſing ordinances and regulations, will be part of the buſineſs of ſuch meeting; nor ſhall any ſtatute, law, regulation or ordinance be then or at any time enacted or paſſed, unleſs thirteen Members of the ſaid Society or ſuch greater number of Members as may be afterwards fixed by the rules of the Society be preſent, beſides ſuch quorum of the officers and council as the laws of the Society for the time being may require, and unleſs the ſame be voted by two-thirds of the whole body then preſent; all which ſtatutes, laws, ordinances and regulations ſo as aforeſaid duly made, enacted and paſſed, ſhall be binding upon every Member of the ſaid Society, and be from time to time inviolably obſerved, according to the tenor and effect thereof; provided they be not repugnant or contrary

trary to the laws of this commonwealth, for the time being in force and effect.

And whereas nations truly civilized (however unhappily at variance on other accounts) will never wage war with the arts and ſciences and the common intereſts of humanity.

Be it further enacted by the authority aforeſaid, That it ſhall and may be lawful for the ſaid Society, by their proper officers, at all times, whether in peace or war, to correſpond with learned ſocieties, as well as individual learned men, of any nation or country, upon matters merely belonging to the buſineſs of the ſaid Society; ſuch as the mutual communication of their diſcoveries and proceedings in philoſophy and ſcience; the procuring books, apparatus, natural curioſities, and ſuch other articles and intelligence as are uſually exchanged between learned bodies for furthering their common purſuits. *Provided always*, That ſuch correſpondence of the ſaid Society be at all times open to the inſpection of the Supreme Executive Council of this commonwealth.

(Signed) JOHN BAYARD, *Speaker.*

Enacted into a Law at Philadelphia, on Wedneſday the fifteenth day of March, Anno Domini one thouſand ſeven hundred and eighty.

(Signed) THOMAS PAINE, *Clerk of the General Aſſembly.*

(Copy)

A Law to encreaſe the annual Subſcriptions of the Members of the American Philoſophical Society held at Philadelphia for promoting uſeful knowledge, and alſo to encreaſe the Depoſites of newly elected Members on their admiſſion into the ſaid Society.

WHEREAS the cuſtomary annual payment of *Ten Shillings* from each Member of the American Philoſophical Society, and alſo the cuſtomary depoſite of *Ten Shillings* by each newly elected Member, on his admiſſion into the ſaid Society, hath been found inadequate to the neceſſary and occaſional expenditures of the Society.

Be it therefore enacted, and it is hereby enacted by the American Philoſophical Society, held at Philadelphia for promoting uſeful knowledge, by virtue of the chartered rights to the ſaid Society granted, and by authority of the ſame, That for the future, that is to ſay, from and after the firſt of March next, the payments to be made by every Member of the ſaid Society ſhall be *Four Dollars* annually, and the depoſite to be made by every newly elected Member, from and after the ſaid firſt of March, ſhall be *Ten Dollars*. And no Member who ſhall be in arrear for his annual ſubſcription or depoſite, ſhall be eligible to any office in the ſaid Society, or be permitted to vote at elections, or at the ordinary meetings of the Society, until he ſhall have fully paid up his ſaid arrears.

And be it further enacted, That no newly elected Member ſhall receive a certificate of his election, or be admitted as a Member of the ſaid Society, until he ſhall have paid into the treaſury the ſaid depoſite of *Ten Dollars* and alſo his arrearages of *Four Dollars* per annum, if any ſuch ſhall have accrued from the time of his election.

Provided always, That honorary Members in foreign parts, ſhall not be ſubject to this law, nor ſhall their certificates of election be with-held on account of their not paying the depoſite or annual ſubſcription aforeſaid. Nevertheleſs, if any ſuch foreign Member ſhould happen to come to the city of Philadelphia with a view of ſettlement or reſidence, then ſuch Member ſhall pay the depoſite money as before directed, and ſhall thenceforth be liable for the annual ſubſcription in common with other reſident Members.

And be it further enacted, That all former laws, ordinances or cuſtoms inconſiſtent with or contradictory to this act, be, and the ſame are hereby repealed.

Enacted into a law at a meeting of the American Philoſophical Society, according to Charter, this ſixth day of January, Anno Domini, one thouſand ſeven hundred and eighty-ſix.

Ex-

Extracts from the Minutes of the American Philoſophical Society, reſpecting a Donation propoſed by Mr. J. H. de MAGELLAN, *of London.*

January 1786,

MR. J. H. de Magellan of London, having in a letter, dated the 17th of September laſt, and communicated to the ſociety by Mr. Vaughan, one of the Vice-Preſidents, made an offer to the ſociety of *two hundred guineas*, to be veſted in a permanent fund, that the intereſt ariſing therefrom may be diſpoſed of in annual premiums, to the authors of the beſt diſcoveries or moſt uſeful improvements relating to navigation, or to natural philoſophy, mere natural hiſtory only excepted: And the ſociety having moſt thankfully accepted the generous offer, appointed a committee to frame rules and conditions for the diſpoſition of the propoſed premiums, agreeable to the intention of the donor, expreſſed in his letter, but more preciſe in the terms; which being done, and approved of by the ſociety, were immediately tranſmitted in a letter to Mr. Magellan, for his confirmation or amendment. They are as follow, viz.

1. The candidate ſhall ſend his diſcovery, invention or improvement, addreſſed to the Preſident or a Vice-Preſident of the Society, free of poſtage or other charges; and ſhall diſtinguiſh his performance by ſome motto, device or ſignature, at his pleaſure. Together with his diſcovery, invention or improvement, he ſhall alſo ſend a ſealed letter, containing the ſame motto, device or ſignature, and ſubſcribed with the real name and place of reſidence of the author.

2. Perſons

2. Perſons of any nation, ſect, or denomination whatever, ſhall be admitted as candidates for this premium.

3. No diſcovery, invention or improvement ſhall be entitled to this premium, which hath been already publiſhed, or for which the author hath been publicly rewarded elſewhere.

4. The candidate ſhall communicate his diſcovery, invention or improvement either in the Engliſh, French, German, or Latin language.

5. All ſuch communications ſhall be publicly read or exhibited to the Society, at ſome ſtated meeting, not leſs than one month previous to the day of adjudication; and ſhall at all times be open to the inſpection of ſuch members as ſhall deſire it. But no member ſhall carry home with him the communication, deſcription or model, except the officer to whom it ſhall be entruſted: nor ſhall ſuch officer part with the ſame out of his cuſtody, to any but the judges, who may demand it for conſideration.

6. The twelve counſellors, together with the other officers annually elected according to the charter and laws of the Society, ſhall be judges of the merits of the ſeveral communications, and award the premium. Which adjudication ſhall be determined by a majority of judges met; provided that ſuch majority be not leſs than ſeven concurring votes.

7. And for this purpoſe the counſellors and other officers, or at leaſt ſeven of them, ſhall meet on the ſecond Monday in December, in every year, to form their judgment and award the premium. After due conſideration had, a vote ſhall firſt be taken on this queſtion, viz. "Whe-" ther any of the communications then under inſpection, " are worthy of the propoſed premium?" If this ſhall be determined in the *negative*, the whole buſineſs ſhall be deferred till another year: But if in the *affirmative*, the judges ſhall then proceed to determine, by vote, the diſcovery, invention or improvement moſt uſeful and worthy.

And

And that diſcovery, invention or improvement which ſhall be found to have the greateſt number of concurring votes (being not leſs than ſeven) in its favour, ſhall be ſucceſsful. Whereupon a certificate in writing ſhall be forthwith drawn of this adjudication, and ſigned by thoſe who voted for the crowned ſubject: And then, *and not till then*, the ſealed letter accompanying the crowned performance, ſhall be opened and the name of the author announced; which certificate ſhall be preſented to the Society at their next ſtated meeting, and delivered to the Secretary to be entered on record, in a bound book provided for this purpoſe.

8. A full account of the crowned ſubject ſhall be publiſhed by the Society as ſoon as may be, after the adjudication, either in a ſeparate publication, or in the next ſucceeding volume of their Tranſactions, or in both.

9. The unſucceſsful performances ſhall lie over for conſideration, and remain, as candidates for the premium, for *five* ſucceeding years next after their preſentment; unleſs the author or authors ſhall think fit to withdraw them or any of them: And the Society ſhall publiſh annually an abſtract of the titles, object or ſubject matter of the communications ſo under conſideration, ſuch only excepted as the counſellors and other officers ſhall, by vote as aforeſaid, have determined not worthy of public notice.

10. No counſellor or officer who is a candidate ſhall ſit in judgment, or give his vote.

11. The letters containing the names of authors whoſe performances ſhall be rejected, or ſhall be found unſucceſsful after a trial of five years, ſhall be burnt without breaking the ſeals.

12. In caſe there ſhould be a failure, in any year, of any communication worthy of the propoſed premium, there will then be two premiums awarded in the next year. But no accumulation of premiums ſhall entitle an author to more than one premium for any one diſcovery, invention or improvement.

13. The

13. The premium ſhall conſiſt of an oval plate of ſolid ſtandard gold, of the value of *Ten Guineas*. On one ſide thereof ſhall be neatly engraved the following motto, ———————— together with theſe words, *The donation of —— of London, eſtabliſhed in the year* 1786. And on the other ſide of the plate ſhall be engraved theſe words, *Awarded by the A. P. S. to —— —— for his diſcovery of —— —— A. D.* 17—. ——— *Preſident.* And the ſeal of the Society ſhall be annexed to the ſaid golden plate, by a ribbon paſſing through a ſmall hole near the lower edge thereof.

LIST

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AMERICAN PHILOSOPHICAL SOCIETY,

Held at PHILADELPHIA, for promoting uſeful Knowledge,

For the Year 1786.

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TRANS-

TRANSACTIONS

OF THE

American PHILOSOPHICAL SOCIETY, *&c.*

N° I.

A Letter from Dr. B. FRANKLIN *to Dr.* INGENHAUSZ, *Phyſician to the Emperor, at Vienna.*

Dear Friend, At ſea, Auguſt 28th, 1785.

Read 21ſt Oct. 1785.

IN one of your letters, a little before I left France, you deſire me to give you in writing my thoughts upon the conſtruction and uſe of chimneys, a ſubject you had ſometimes heard me touch upon in converſation. I embrace willingly this leiſure afforded by my preſent ſituation to comply with your requeſt, as it will not only ſhow my regard to the deſires of a friend, but may at the ſame time be of ſome utility to others; the doctrine of chimneys appearing not to be as yet generally well underſtood, and miſtakes reſpecting them being attended with conſtant inconvenience, if not remedied; and with fruitleſs expence, if the true remedies are miſtaken.

Thoſe who would be acquainted with this ſubject ſhould begin by conſidering on what principle ſmoke aſcends in any chimney. At firſt many are apt to think that ſmoke

is

is in its nature and of itſelf ſpecifically lighter than air, and riſes in it for the ſame reaſon that cork riſes in water. Theſe ſee no cauſe why ſmoke ſhould not riſe in the chimney, though the room be ever ſo cloſe. Others think there is a power in chimneys to *draw* up the ſmoke, and that there are different forms of chimneys which afford more or leſs of this power. Theſe amuſe themſelves with ſearching for the beſt form. The equal dimenſions of a funnel in its whole length is not thought artificial enough, and it is made, for fancied reaſons, ſometimes tapering and narrowing from below upwards, and ſometimes the contrary, &c. &c. A ſimple experiment or two may ſerve to give more correct ideas. Having lit a pipe of tobacco, plunge the ſtem to the bottom of a decanter half filled with cold water; then putting a rag over the bowl, blow through it and make the ſmoke deſcend in the ſtem of the pipe, from the end of which it will riſe in bubbles through the water; and being thus cooled, will not afterwards riſe to go out through the neck of the decanter, but remain ſpreading itſelf and reſting on the ſurface of the water. This ſhows that ſmoke is really heavier than air, and that it is carried upwards only when attached to, or acted upon, by air that is heated, and thereby rarefied and rendered ſpecifically lighter than the air in its neighbourhood.

Smoke being rarely ſeen but in company with heated air, and its upward motion being viſible, though that of the rarefied air that drives it is not ſo, has naturally given riſe to the error.

I need not explain to you, my learned friend, what is meant by rarefied air; but if you make the public uſe you propoſe of this letter, it may fall into the hands of ſome who are unacquainted with the term and with the thing. Theſe then may be told, that air is a fluid which has weight as well as others, though about eight hundred times lighter than water. That heat makes the particles of air recede

recede from each other and take up more ſpace, ſo that the ſame weight of air heated will have more bulk, than equal weights of cold air which may ſurround it, and in that caſe muſt riſe, being forced upwards by ſuch colder and heavier air, which preſſes to get under it and take its place. That air is ſo rarefied or expanded by heat, may be proved to their comprehenſion by a lank blown bladder, which laid before a fire will ſoon ſwell, grow tight and burſt.

Another experiment may be to take a glaſs tube about an inch in diameter, and twelve inches long, open at both ends and fixed upright on legs ſo that it need not be handled, for the hands might warm it. At the end of a quill faſten five or ſix inches of the fineſt light filament of ſilk, ſo that it may be held either above the upper end of the tube or under the lower end, your warm hand being at a diſtance by the length of the quill. If there were any motion of air through the tube, it would manifeſt itſelf by its effect on the ſilk; but if the tube and the air in it are of the ſame temperature with the ſurrounding air, there will be no ſuch motion, whatever may be the form of the tube, whether crooked or ſtrait, narrow below and widening upwards, or the contrary; the air in it will be quieſcent. Warm the tube, and you will find as long as it continues warm, a conſtant current of air entering below and paſſing up through it, till diſcharged at the top; becauſe the warmth of the tube being communicated to the air it contains, rarefies that air and makes it lighter than the air without, which therefore preſſes in below, forces it upwards, follows and takes its place, and is rarefied in its turn. And, without warming the tube, if you hold under it a knob of hot iron, the air thereby heated will riſe and fill the tube, going out at its top, and this motion in the tube will continue as long as the knob remains hot, becauſe the air entering the tube below is heated and rarefied by paſſing near and over that knob. That

Plate I. Figure I.

That this motion is produced merely by the difference of ſpecific gravity between the fluid within and that without the tube, and not by any fancied form of the tube itſelf, may appear by plunging it into water contained in a glaſs jar a foot deep, through which ſuch motion might be ſeen. The water within and without the tube being of the ſame ſpecific gravity, balance each other, and both remain at reſt. But take out the tube, ſtop its bottom with a finger and fill it with olive oil, which is lighter than water, then ſtopping the top, place it as before, its lower end under water, its top a very little above. As long as you keep the bottom ſtopt, the fluids remain at reſt, but the moment it is unſtopt, the heavier enters below, forces up the lighter, and takes its place. And the motion then ceaſes, merely becauſe the new fluid cannot be ſucceſſively made lighter, as air may be by a warm tube.

In fact, no form of the funnel of a chimney has any ſhare in its operation or effect reſpecting ſmoke, except its height. The longer the funnel, if erect, the greater its force when filled with heated and rarefied air, to *draw* in below and drive up the ſmoke, if one may, in compliance with cuſtom, uſe the expreſſion *draw*, when in fact it is the ſuperior weight of the ſurrounding atmoſphere that *preſſes* to enter the funnel below, and ſo *drives up* before it the ſmoke and warm air it meets with in its paſſage.

I have been the more particular in explaining theſe firſt principles, becauſe, for want of clear ideas reſpecting them, much fruitleſs expence has been occaſioned; not only ſingle chimneys, but in ſome inſtances, within my knowledge, whole ſtacks having been pulled down and rebuilt with funnels of different forms, imagined more powerful in *drawing* ſmoke; but having ſtill the ſame height and the ſame opening below, have performed no better than their predeceſſors.

What is it then which makes a *ſmoky chimney*, that is, a chimney which inſtead of conveying up all the ſmoke, diſcharges

diſcharges a part of it into the room, offending the eyes and damaging the furniture?

The cauſes of this effect, which have fallen under my obſervation, amount to *nine*, differing from each other, and therefore requiring different remedies.

1. *Smoky chimneys in a new houſe, are ſuch, frequently from mere want of air.* The workmanſhip of the rooms being all good, and juſt out of the workman's hand, the joints of the boards of the flooring, and of the pannels of wainſcotting are all true and tight, the more ſo as the walls, perhaps not yet thoroughly dry, preſerve a dampneſs in the air of the room which keeps the wood-work ſwelled and cloſe. The doors and the ſaſhes too, being worked with truth, ſhut with exactneſs, ſo that the room is as tight as a ſnuff-box, no paſſage being left open for air to enter, except the key-hole, and even that is ſometimes covered by a little dropping ſhutter. Now if ſmoke cannot riſe but as connected with rarefied air, and a column of ſuch air, ſuppoſe it filling the funnel, cannot riſe, unleſs other air be admitted to ſupply its place; and if, therefore, no current of air enter the opening of the chimney, there is nothing to prevent the ſmoke coming out into the room. If the motion upwards of the air in a chimney that is freely ſupplied, be obſerved by the riſing of the ſmoke or a feather in it, and it be conſidered that in the time ſuch feather takes in riſing from the fire to the top of the chimney, a column of air equal to the content of the funnel muſt be diſcharged, and an equal quantity ſupplied from the room below, it will appear abſolutely impoſſible that this operation ſhould go on if the tight room is kept ſhut; for were there any force capable of drawing conſtantly ſo much air out of it, it muſt ſoon be exhauſted like the receiver of an air pump, and no animal could live in it. Thoſe therefore who ſtop every crevice in a room to prevent the admiſſion of freſh air, and yet would have their chimney carry up the ſmoke, require inconſiſtencies,

and

and expect impoſſibilities. Yet under this ſituation, I have ſeen the owner of a new houſe, in deſpair, and ready to ſell it for much leſs than it coſt, conceiving it uninhabitable, becauſe not a chimney in any one of its rooms would carry off the ſmoke, unleſs a door or window were left open. Much expence has alſo been made, to alter and amend new chimneys which had really no fault; in one houſe particularly that I knew, of a nobleman in Weſtminſter, that expence amounted to no leſs than three hundred pounds, *after* his houſe had been, as he thought, finiſhed and all charges paid. And after all, ſeveral of the alterations were ineffectual, for want of underſtanding the true principles.

Remedies. When you find on trial, that opening the door or a window, enables the chimney to carry up all the ſmoke, you may be ſure that want of air *from without*, was the cauſe of its ſmoking. I ſay *from without*, to guard you againſt a common miſtake of thoſe who may tell you, the room is large, contains abundance of air, ſufficient to ſupply any chimney, and therefore it cannot be that the chimney wants air. Theſe reaſoners are ignorant, that the largeneſs of a room, if tight, is in this caſe of ſmall importance, ſince it cannot part with a chimney full of its air without occaſioning ſo much vacuum; which it requires a great force to effect, and could not be borne if effected.

It appearing plainly, then, that ſome of the outward air muſt be admitted, the queſtion will be, how much is *abſolutely neceſſary*; for you would avoid admitting more, as being contrary to one of your intentions in having a fire, viz. that of warming your room. To diſcover this quantity, ſhut the door gradually while a middling fire is burning, till you find that, before it is quite ſhut, the ſmoke begins to come out into the room, then open it a little till you perceive the ſmoke comes out no longer. There hold the door, and obſerve the width of the open crevice between

tween the edge of the door and the rabbit it ſhould ſhut into. Suppoſe the diſtance to be half an inch, and the door eight feet high, you find thence that your room requires an entrance for air equal in area to ninety ſix half inches, or forty eight ſquare inches, or a paſſage of ſix inches by eight. This however is a large ſuppoſition, there being few chimneys, that, having a moderate opening and a tolerable height of funnel, will not be ſatisfied with ſuch a crevice of a quarter of an inch; and I have found a ſquare of ſix by ſix, or thirty ſix ſquare inches, to be a pretty good medium, that will ſerve for moſt chimneys. High funnels with ſmall and low openings, may indeed be ſupplied through a leſs ſpace, becauſe, for reaſons that will appear hereafter, the *force of levity*, if one may ſo ſpeak, being greater in ſuch funnels, the cool air enters the room with greater velocity, and conſequently more enters in the ſame time. This however has its limits, for experience ſhows that no increaſed velocity ſo occaſioned, has made the admiſſion of air through the key-hole equal in quantity to that through an open door; though through the door the current moves ſlowly, and through the key-hole with great rapidity.

It remains then to be conſidered how and where this neceſſary quantity of air from without is to be admitted ſo as to be leaſt inconvenient. For, if at the door, left ſo much open, the air thence proceeds directly to the chimney, and in its way comes cold to your back and heels as you ſit before your fire. If you keep the door ſhut, and raiſe a little the ſaſh of your window, you feel the ſame inconvenience. Various have been the contrivances to avoid this, ſuch as bringing in freſh air through pipes in the jams of the chimney, which pointing upwards ſhould blow the ſmoke up the funnel; opening paſſages into the funnel above, to let in air for the ſame purpoſe. But theſe produce an effect contrary to that intended: For as it is the conſtant current of air paſſing from the room *through the*

the opening of the chimney into the funnel which prevents the ſmoke coming out into the room, if you ſupply the funnel by other means or in other ways with the air it wants, and eſpecially if that air be cold, you diminiſh the force of that current, and the ſmoke in its efforts to enter the room finds leſs reſiſtance.

The wanted air muſt then *indiſpenſably* be admitted into the room, to ſupply what goes off through the opening of the chimney. M. Gauger, a very ingenious and intelligent French writer on the ſubject, propoſes with judgment to admit it *above* the opening of the chimney; and to prevent inconvenience from its coldneſs, he directs its being made to paſs in its entrance through winding cavities made behind the iron back and ſides of the fire-place, and under the iron hearth-plate; in which cavities it will be warmed, and even heated, ſo as to contribute much, inſtead of cooling, to the warming of the room. This invention is excellent in itſelf, and may be uſed with advantage in building new houſes; becauſe the chimneys may then be ſo diſpoſed, as to admit conveniently the cold air to enter ſuch paſſages: But in houſes built without ſuch views, the chimneys are often ſo ſituated, as not to afford that convenience, without great and expenſive alterations. Eaſy and cheap methods, though not quite ſo perfect in themſelves, are of more general utility; and ſuch are the following.

In all rooms where there is a fire, the body of air warmed and rarefied before the chimney is continually changing place, and making room for other air that is to be warmed in its turn. Part of it enters and goes up the chimney, and the reſt riſes and takes place near the ceiling. If the room be lofty, that warm air remains above our heads as long as it continues warm, and we are little benefited by it, becauſe it does not deſcend till it is cooler. Few can imagine the difference of climate between the upper and lower parts of ſuch a room, who have not tried it by the thermometer,

thermometer, or by going up a ladder till their heads are near the ceiling. It is then among this warm air that the wanted quantity of outward air is beſt admitted, with which being mixed, its coldneſs is abated, and its inconvenience diminiſhed ſo as to become ſcarce obſervable. This may be eaſily done, by drawing down about an inch the upper ſaſh of a window; or, if not moveable, by cutting ſuch a crevice through its frame; in both which caſes, it will be well to place a thin ſhelf of the length, to conceal the opening, and ſloping upwards to direct the entering air horizontally along and under the ceiling. In ſome houſes the air may be admitted by ſuch a crevice made in the wainſcot, corniſh or plaſtering, near the ceiling and over the opening of the chimney. This, if practicable, is to be choſen, becauſe the entering cold air will there meet with the warmeſt riſing air from before the fire, and be ſooneſt tempered by the mixture. The ſame kind of ſhelf ſhould alſo be placed here. Another way, and not a very difficult one, is to take out an upper pane of glaſs in one of your ſaſhes, ſet it in a tin frame, giving it two ſpringing angular ſides, and then replacing it, with hinges below on which it may be turned to open more or leſs above. It will then have the appearance of an internal ſky light. By drawing this pane in, more or leſs, you may admit what air you find neceſſary. Its poſition will naturally throw that air up and along the ceiling. This is what is called in France a *Was iſt das?* As this is a German queſtion, the invention is probably of that nation, and takes its name from the frequent aſking of that queſtion when it firſt appeared. In England, ſome have of late years cut a round hole about five inches diameter in a pane of the ſaſh and placed againſt it a circular plate of tin hung on an axis, and cut into vanes, which being ſeparately bent a little obliquely, are acted upon by the entering air, ſo as to force the plate continually round like the vanes of a windmill. This ad-

Plate I. Figure 2.

mits the outward air, and by the continual whirling of the vanes, does in ſome degree diſperſe it. The noiſe only, is a little inconvenient.

2. A ſecond cauſe of the ſmoking of chimneys is, *their openings in the room being too large;* that is, too wide, too high or both. Architects in general have no other ideas of proportion in the opening of a chimney, than what relate to ſymmetry and beauty, reſpecting the dimenſions of the room*; while its true proportion, reſpecting its function and utility depends on quite other principles; and they might as properly proportion the ſtep in a ſtair-caſe to the height of the ſtory, inſtead of the natural elevation of men's legs in mounting. The proportion then to be regarded, is what relates to the height of the funnel. For as the funnels in the different ſtories of a houſe are neceſſarily of different heights or lengths, that from the loweſt floor being the higheſt or longeſt, and thoſe of the other floors ſhorter and ſhorter, till we come to thoſe in the garrets, which are of courſe the ſhorteſt; and the force of draft being, as already ſaid, in proportion to the height of funnel filled with rarefied air; and a current of air from the room into the chimney, ſufficient to fill the opening, being neceſſary to oppoſe and prevent the ſmoke coming out into the room; it follows that the openings of the longeſt funnels may be larger, and that thoſe of the ſhorter funnels ſhould be ſmaller. For if there be a large opening to a chimney that does not draw ſtrongly, the funnel may happen to be furniſhed with the air it demands by a partial current entering on one ſide of the opening, and leaving the other ſide free of any oppoſing current, may permit the ſmoke to iſſue there into the room. Much too of the force of draft in a funnel depends on the degree of rarefaction in the air it contains, and that depends on the nearneſs to the fire of its paſſage in entering the funnel. If it can enter far from the fire on each ſide, or far above the fire, in a wide or high opening, it receives little heat in

* See Appendix, N° I.

in paſſing by the fire, and the contents of the funnel is by that means leſs different in levity from the ſurrounding atmoſphere, and its force in drawing conſequently weaker. Hence if too large an opening be given to chimneys in upper rooms, thoſe rooms will be ſmoky: On the other hand, if too ſmall openings be given to chimneys in the lower rooms, the entering air operating too directly and violently on the fire, and afterwards ſtrengthening the draft as it aſcends the funnel, will conſume the fuel too rapidly.

Remedy. As different circumſtances frequently mix themſelves in theſe matters, it is difficult to give preciſe dimenſions for the openings of all chimneys. Our fathers made them generally much too large; we have leſſened them; but they are often ſtill of greater dimenſion than they ſhould be, the human eye not being eaſily reconciled to ſudden and great changes. If you ſuſpect that your chimney ſmokes from the too great dimenſion of its opening, contract it by placing moveable boards ſo as to lower and narrow it gradually, till you find the ſmoke no longer iſſues into the room. The proportion ſo found will be that which is proper for that chimney, and you may employ the bricklayer or maſon to reduce it accordingly. However, as, in building new houſes, ſomething muſt be ſometimes hazarded, I would make the openings in my lower rooms about thirty inches ſquare and eighteen deep, and thoſe in the upper, only eighteen inches ſquare and not quite ſo deep; the intermediate ones diminiſhing in proportion as the height of funnel diminiſhed. In the larger openings, billets of two feet long, or half the common length of cord-wood, may be burnt conveniently; and for the ſmaller, ſuch wood may be ſawed into thirds. Where coals are the fuel, the grates will be proportioned to the openings. The ſame depth is nearly neceſſary to all, the funnels being all made of a ſize proper to admit a chimney-ſweeper. If in large and elegant rooms cuſtom or fancy ſhould re-

quire the appearance of a larger chimney, it may be formed of expenſive marginal decorations, in marble, &c. In time perhaps that which is fitteſt in the nature of things, may come to be thought handſomeſt. But at preſent when men and women in different countries ſhow themſelves diſſatisfied with the forms God has given to their heads, waiſts and feet, and pretend to ſhape them more perfectly, it is hardly to be expected that they will be content always with the beſt form of a chimney. And there are ſome I know ſo bigotted to the fancy of a large noble opening, that rather than change it, they would ſubmit to have damaged furniture, ſore eyes and ſkins almoſt ſmoked to bacon.

3. Another cauſe of ſmoky chimneys is, *too ſhort a funnel.* This happens neceſſarily in ſome caſes, as where a chimney is required in a low building; for, if the funnel be raiſed high above the roof, in order to ſtrengthen its draft, it is then in danger of being blown down, and cruſhing the roof in its fall.

Remedies. Contract the opening of the chimney, ſo as to oblige all the entering air to paſs through or very near the fire; whereby it will be more heated and rarefied, the funnel itſelf be more warmed, and its contents have more of what may be called the force of levity, ſo as to riſe ſtrongly and maintain a good draft at the opening.

Or you may in ſome caſes, to advantage, build additional ſtories over the low building, which will ſupport a high funnel.

If the low building be uſed as a kitchen, and a contraction of the opening therefore inconvenient, a large one being neceſſary, at leaſt when there are great dinners, for the free management of ſo many cooking utenſils; in ſuch caſe I would adviſe the building of two more funnels joining to the firſt, and having three moderate openings, one to each funnel, inſtead of one large one. When there is occaſion to uſe but one, the other two may be kept ſhut by

by ſliding plates, hereafter to be deſcribed*; and two or all of them may be uſed together when wanted. This will indeed be an expence, but not an uſeleſs one, ſince your cooks will work with more comfort, ſee better than in a ſmoky kitchen what they are about, your victuals will be cleaner dreſſed and not taſte of ſmoke, as is often the caſe; and to render the effect more certain, a ſtack of three funnels may be ſafely built higher above the roof than a ſingle funnel.

The caſe of too ſhort a funnel is more general than would be imagined, and often found where one would not expect it. For it is not uncommon, in ill-contrived buildings, inſtead of having a funnel for each room or fireplace, to bend and turn the funnel of an upper room ſo as to make it enter the ſide of another funnel that comes from below. By this means the upper room funnel is made ſhort of courſe, ſince its length can only be reckoned from the place where it enters the lower room funnel; and that funnel is alſo ſhortened by all the diſtance between the entrance of the ſecond funnel and the top of the ſtack: For all that part being readily ſupplied with air through the ſecond funnel, adds no ſtrength to the draft, eſpecially as that air is cold when there is no fire in the ſecond chimney. The only eaſy remedy here is, to keep the opening ſhut of that funnel in which there is no fire.

4. Another very common cauſe of the ſmoking of chimneys, is, *their overpowering one another*. For inſtance, if there be two chimneys in one large room, and you make fires in both of them, the doors and windows cloſe ſhut, you will find that the greater and ſtronger fire ſhall overpower the weaker, and draw air down its funnel to ſupply its own demand; which air deſcending in the weaker funnel will drive down its ſmoke, and force it into the room. If, inſtead of being in one room, the two chimneys are in two different rooms, communicating by a door, the caſe is the ſame whenever that door is open. In a very tight

* See Appendix, N° II.

tight houſe, I have known a kitchen chimney on the loweſt floor, when it had a great fire in it, overpower any other chimney in the houſe, and draw air and ſmoke into its room, as often as the door was opened communicating with the ſtaircaſe.

Remedy. Take care that every room have the means of ſupplying itſelf from without, with the air its chimney may require, ſo that no one of them may be obliged to borrow from another, nor under the neceſſity of lending. A variety of theſe means have been already deſcribed.

5. Another cauſe of ſmoking is, *when the tops of chimneys are commanded by higher buildings, or by a hill,* ſo that the wind blowing over ſuch eminences falls like water over a dam, ſometimes almoſt perpendicularly on the tops of the chimneys that lie in its way, and beats down the ſmoke contained in them.

Remedy. That commonly applied to this caſe, is a turncap made of tin or plate iron, covering the chimney above and on three ſides, open on one ſide, turning on a ſpindle, and which being guided or governed by a vane, always preſents its back to the current. This I believe may be generally effectual, though not certain, as there may be caſes in which it will not ſucceed. Raiſing your funnels if practicable, ſo as their tops may be higher, or at leaſt equal with the commanding eminence, is more to be depended on. But the turning cap, being eaſier and cheaper, ſhould firſt be tried. If obliged to build in ſuch a ſituation, I would chuſe to place my doors on the ſide next the hill, and the backs of my chimneys on the furtheſt ſide; for then the column of air falling over the eminence, and of courſe preſſing on that below and forcing it to enter the doors, or *Was-iſt-das*es on that ſide, would tend to balance the preſſure down the chimneys, and leave the funnels more free in the exerciſe of their functions.

6. There is another caſe of command, the reverſe of that laſt mentioned. It is where the commanding eminence

nence is farther from the wind than the chimney commanded. To explain this a figure may be neceſſary. Suppoſe then a building whoſe ſide A, happens to be expoſed to the wind, and forms a kind of dam againſt its progreſs. The air obſtructed by this dam will like water preſs and ſearch for paſſages through it; and finding the top of the chimney B, below the top of the dam, it will force itſelf down that funnel, in order to get through by ſome door or window open on the other ſide of the building. And if there be a fire in ſuch chimney, its ſmoke is of courſe beat down, and fills the room. Plate I. Figure 3.

Remedy. I know of but one, which is to raiſe ſuch funnel higher than the roof, ſupporting it, if neceſſary, by iron bars. For a turn-cap in this caſe has no effect, the dammed up air preſſing down through it in whatever poſition the wind may have placed its opening.

I know a city in which many houſes are rendered ſmoky by this operation. For their kitchens being built behind, and connected by a paſſage with the houſes, and the tops of the kitchen chimneys lower than the top of the houſes, the whole ſide of a ſtreet when the wind blows againſt its back, forms ſuch a dam as above deſcribed; and the wind ſo obſtructed forces down thoſe kitchen chimneys, (eſpecially when they have but weak fires in them) to paſs through the paſſage and houſe, into the ſtreet. Kitchen chimneys ſo formed and ſituated, have another inconvenience. In ſummer, if you open your upper room windows for air, a light breeze blowing over your kitchen chimney towards the houſe, though not ſtrong enough to force down its ſmoke as aforeſaid, is ſufficient to waft it into your windows, and fill the rooms with it; which, beſides the diſagreeableneſs, damages your furniture.

7. Chimneys, otherwiſe drawing well, are ſometimes made to ſmoke by *the improper and inconvenient ſituation of a door.* When the door and chimney are on the ſame ſide of the room as in the figure, if the door A, being in the

Plate I. Figure 4.

the corner is made to open againſt the wall, which is common, as being there, when open, more out of the way, it follows, that when the door is only opened in part, a current of air ruſhing in paſſes along the wall into and acroſs the opening of the chimney B, and flirts ſome of the ſmoke out into the room. This happens more certainly when the door is ſhutting, for then the force of the current is augmented, and becomes very inconvenient to thoſe who, warming themſelves by the fire, happen to ſit in its way.

The *Remedies* are obvious and eaſy. Either put an intervening ſkreen from the wall round great part of the fireplace; or, which is perhaps preferable, ſhift the hinges of your door, ſo as it may open the other way, and when open throw the air along the other wall.

8. A room that has no fire in its chimney, is ſometimes filled with *ſmoke which is received at the top of its funnel and deſcends into the room.* In a former paper * I have already explained the deſcending currents of air in cold funnels; it may not be amiſs however to repeat here, that funnels without fires have an effect according to their degree of coldneſs or warmth, on the air that happens to be contained in them. The ſurrounding atmoſphere is frequently changing its temperature; but ſtacks of funnels covered from winds and ſun by the houſe that contains them, retain a more equal temperature. If, after a warm ſeaſon, the outward air ſuddenly grows cold, the empty warm funnels begin to draw ſtrongly upward; that is, they rarefy the air contained in them, which of courſe riſes, cooler air enters below to ſupply its place, is rarefied in its turn and riſes; and this operation continues, till the funnel grows cooler, or the outward air warmer, or both, when the motion ceaſes. On the other hand, if after a cold ſeaſon, the outward air ſuddenly grows warm and of courſe lighter, the air contained in the cool funnels, being heavier, deſcends into the room; and the warmer

* See Appendix, N° II.

warmer air which enters their tops being cooled in its turn, and made heavier, continues to deſcend; and this operation goes on, till the funnels are warmed by the paſſing of warm air through them, or the air itſelf grows cooler. When the temperature of the air and of the funnels is nearly equal, the difference of warmth in the air between day and night is ſufficient to produce theſe currents, the air will begin to aſcend the funnels as the cool of the evening comes on, and this current will continue till perhaps nine or ten o'clock the next morning, when it begins to heſitate; and as the heat of the day approaches, it ſets downwards, and continues ſo till towards evening, when it again heſitates for ſome time, and then goes upwards conſtantly during the night, as before mentioned. Now when ſmoke iſſuing from the tops of neighbouring funnels paſſes over the tops of funnels which are at the time drawing downwards, as they often are in the middle part of the day, ſuch ſmoke is of neceſſity drawn into theſe funnels, and deſcends with the air into the chamber.

The *Remedy* is to have a ſliding plate, hereafter deſcribed*, that will ſhut perfectly the offending funnel.

9. Chimneys which generally draw well, do nevertheleſs ſometimes give ſmoke into the rooms, *it being driven down by ſtrong winds paſſing over the tops of their funnels*, though not deſcending from any commanding eminence. This caſe is moſt frequent where the funnel is ſhort, and the opening turned from the wind. It is the more grievous, when it happens to be a cold wind that produces the effect, becauſe when you moſt want your fire, you are ſometimes obliged to extinguiſh it. To underſtand this, it may be conſidered that the riſing light air, to obtain a free iſſue from the funnel, muſt puſh out of its way or oblige the air that is over it to riſe. In a time of calm or of little wind this is done viſibly, for we ſee the ſmoke that is brought up by that air riſe in a column above the chimney. But when a violent current of air, that is, a

* See Appendix, N° II.

ſtrong wind, paſſes over the top of a chimney, its particles have received ſo much force, which keeps them in a horizontal direction and follow each other ſo rapidly, that the riſing light air has not ſtrength ſufficient to oblige them to quit that direction and move upwards to permit its iſſue. Add to this, that ſome of the current paſſing over that ſide of the funnel which it firſt meets with, viz. at A, having been compreſſed by the reſiſtance of the funnel, may expand itſelf over the flue, and ſtrike the interior oppoſite ſide at B, from whence it may be reflected downwards and from ſide to ſide in the direction of the pricked lines c c c.

Plate I. Figure 5.

Remedies. In ſome places, particularly in Venice, where they have not ſtacks of chimneys but ſingle flues, the cuſtom is, to open or widen the top of the flue rounding in the true form of a funnel; which ſome think may prevent the effect juſt mentioned, for that the wind blowing over one of the edges into the funnel may be ſlanted out again on the other ſide by its form. I have had no experience of this; but I have lived in a windy country, where the contrary is practiſed, the tops of the flues being *narrowed* inwards, ſo as to form a ſlit for the iſſue of the ſmoke, long as the breadth of the funnel, and only four inches wide. This ſeems to have been contrived on a ſuppoſition that the entry of the wind would thereby be obſtructed, and perhaps it might have been imagined, that the whole force of the riſing warm air being condenſed, as it were, in the narrow opening, would thereby be ſtrengthened, ſo as to overcome the reſiſtance of the wind. This however did not always ſucceed; for when the wind was at north-eaſt and blew freſh, the ſmoke was forced down by fits into the room I commonly ſat in, ſo as to oblige me to ſhift the fire into another. The poſition of the ſlit of this funnel was indeed north-eaſt and ſouth-weſt. Perhaps if it had lain acroſs the wind, the effect might have been different. But on this I can give no certainty.

Plate I. Figure 6.

tainty. It ſeems a matter proper to be referred to experiment. Poſſibly a turn-cap might have been ſerviceable, but it was not tried.

Chimneys have not been long in uſe in England. I formerly ſaw a book printed in the time of queen Elizabeth, which remarked the then modern improvements of living, and mentioned among others the convenience of chimneys. "Our forefathers," ſaid the author, "had no chimneys. "There was in each dwelling houſe only one place for a "fire, and the ſmoke went out through a hole in the "roof; but now there is ſcarce a gentleman's houſe in "England that has not at leaſt one chimney in it."—When there was but one chimney, its top might then be opened as a funnel, and perhaps, borrowing the form from the Venetians, it was then the flue of a chimney got that name. Such is now the growth of luxury, that in both England and France we muſt have a chimney for every room, and in ſome houſes every poſſeſſor of a chamber, and almoſt every ſervant, will have a fire; ſo that the flues being neceſſarily built in ſtacks, the opening of each as a funnel is impracticable. This change of manners ſoon conſumed the firewood of England, and will ſoon render fuel extremely ſcarce and dear in France, if the uſe of coals be not introduced in the latter, kingdom as it has been in the former, where it at firſt met with oppoſition; for there is extant in the records of one of queen Elizabeth's parliaments, a motion made by a member, reciting, "that "many dyers, brewers, ſmiths, and other artificers of Lon-"don, had of late taken to the uſe of pitcoal for their fires, "inſtead of wood, which filled the air with noxious va-"pours and ſmoke, very prejudicial to the health, parti-"cularly of perſons coming out of the country; and there-"fore moving that a law might paſs to prohibit the uſe "of ſuch fuel (at leaſt during the ſeſſion of parliament) "by thoſe artificers."—It ſeems it was not then commonly uſed in private houſes. Its ſuppoſed unwholeſomeneſs

was an objection. Luckily the inhabitants of London have got over that objection, and now think it rather contributes to render their air ſalubrious, as they have had no general peſtilential diſorder ſince the general uſe of coals, when, before it, ſuch were frequent. Paris ſtill burns wood at an enormous expence continually augmenting, the inhabitants having ſtill that prejudice to overcome. In Germany you are happy in the uſe of ſtoves, which ſave fuel wonderfully: Your people are very ingenious in the management of fire; but they may ſtill learn ſomething in that art from the Chineſe*, whoſe country being greatly populous and fully cultivated, has little room left for the growth of wood, and having not much other fuel that is good, have been forced upon many inventions during a courſe of ages, for making a little fire go as far as poſſible.

I have thus gone through all the common cauſes of the ſmoking of chimneys that I can at preſent recollect as having fallen under my obſervation; communicating the remedies that I have known ſucceſsfully uſed for the different caſes, together with the principles on which both the diſeaſe and the remedy depend, and confeſſing my ignorance wherever I have been ſenſible of it. You will do well, if you publiſh, as you propoſe, this letter, to add in notes, or as you pleaſe, ſuch obſervations as may have occurred to your attentive mind; and if other philoſophers will do the ſame, this part of ſcience, though humble, yet of great utility, may in time be perfected. For many years paſt, I have rarely met with a caſe of a ſmoky chimney, which has not been ſolvable on theſe principles, and cured by theſe remedies, where people have been willing to apply them; which is indeed not always the caſe; for many have prejudices in favour of the noſtrums of pretending chimney-doctors and fumiſts, and ſome have conceits and fancies of their own, which they rather chuſe to try, than to lengthen a funnel, alter the ſize of an opening, or admit air into a room, however neceſſary; for ſome are as much

* See Appendix, N° III.

much afraid of freſh air as perſons in the hydrophobia are of freſh water. I myſelf had formerly this prejudice, this *aerophobia*, as I now account it, and dreading the ſuppoſed dangerous effects of cool air, I conſidered it as an enemy, and cloſed with extreme care every crevice in the rooms I inhabited. Experience has convinced me of my error. I now look upon freſh air as a friend: I even ſleep with an open window. I am perſuaded that no common air from without, is ſo unwholeſome as the air within a cloſe room that has been often breathed and not changed. Moiſt air too, which formerly I thought pernicious, gives me now no apprehenſions: For conſidering that no dampneſs of air applied to the outſide of my ſkin, can be equal to what is applied to and touches it within, my whole body being full of moiſture, and finding that I can lie two hours in a bath twice a week, covered with water, which certainly is much damper than any air can be, and this for years together, without catching cold, or being in any other manner diſordered by it, I no longer dread mere moiſture, either in air or in ſheets or ſhirts: And I find it of importance to the happineſs of life, the being freed from vain terrors, eſpecially of objects that we are every day expoſed inevitably to meet with. You phyſicians have of late happily diſcovered, after a contrary opinion had prevailed ſome ages, that freſh and cool air does good to perſons in the ſmall pox and other fevers. It is to be hoped that in another century or two we may all find out, that it is not bad even for people in health. And as to moiſt air, here I am at this preſent writing in a ſhip with above forty perſons, who have had no other but moiſt air to breathe for ſix weeks paſt; every thing we touch is damp, and nothing dries, yet we are all as healthy as we ſhould be on the mountains of Switzerland, whoſe inhabitants are not more ſo than thoſe of Bermuda or St. Helena, iſlands on whoſe rocks the waves are daſhed into millions of particles, which fill the air with damp, but produce no diſeaſes

eaſes, the moiſture being pure, unmixed with the poiſonous vapours ariſing from putrid marſhes and ſtagnant pools, in which many inſects die and corrupt the water. Theſe places only, in my opinion, (which however I ſubmit to yours) afford unwholeſome air; and that it is not the mere water contained in damp air, but the volatile particles of corrupted animal matter mixed with that water, which renders ſuch air pernicious to thoſe who breathe it. And I imagine it a cauſe of the ſame kind that renders the air in cloſe rooms, where the perſpirable matter is breathed over and over again by a number of aſſembled people, ſo hurtful to health. After being in ſuch a ſituation, many find themſelves affected by that *febricula*, which the Engliſh alone call *a cold*, and, perhaps from the name, imagine that they caught the malady by *going out* of the room, when it was in fact by being in it.

You begin to think that I wander from my ſubject, and go out of my depth. So I return again to my chimneys.

We have of late many lecturers in experimental philoſophy. I have wiſhed that ſome of them would ſtudy this branch of that ſcience, and give experiments in it as a part of their lectures. The addition to their preſent apparatus need not be very expenſive. A number of little repreſentations of rooms compoſed each of five panes of ſaſh glaſs, framed in wood at the corners, with proportionable doors, and moveable glaſs chimneys, with openings of different ſizes, and different lengths of funnel, and ſome of the rooms ſo contrived as to communicate on occaſion with others, ſo as to form different combinations, and exemplify different caſes; with quantities of green wax taper cut into pieces of an inch and half, ſixteen of which ſtuck together in a ſquare, and lit, would make a ſtrong fire for a little glaſs chimney, and blown out would continue to burn and give ſmoke as long as deſired. With ſuch an apparatus all the operations of ſmoke and rarefied air in rooms and chimneys might be ſeen through their tranſparent

rent ſides; and the effect of winds on chimneys, commanded or otherwiſe, might be ſhown by letting the entering air blow upon them through an opened window of the lecturer's chamber, where it would be conſtant while he kept a good fire in his chimney. By the help of ſuch lectures our fumiſts would become better inſtructed. At preſent they have generally but one remedy, which perhaps they have known effectual in ſome one caſe of ſmoky chimneys, and they apply that indiſcriminately to all the other caſes, without ſucceſs,—but not without expence to their employers.

With all the ſcience, however, that a man ſhall ſuppoſe himſelf poſſeſſed of in this article, he may ſometimes meet with caſes that ſhall puzzle him. I once lodged in a houſe at London, which, in a little room, had a ſingle chimney and funnel. The opening was very ſmall, yet it did not keep in the ſmoke, and all attempts to have a fire in this room were fruitleſs. I could not imagine the reaſon, till at length obſerving that the chamber over it, which had no fire-place in it, was always filled with ſmoke when a fire was kindled below, and that the ſmoke came through the cracks and crevices of the wainſcot; I had the wainſcot taken down, and diſcovered that the funnel which went up behind it, had a crack many feet in length, and wide enough to admit my arm, a breach very dangerous with regard to fire, and occaſioned probably by an apparent irregular ſettling of one ſide of the houſe. The air entering this breach freely, deſtroyed the drawing force of the funnel. The remedy would have been, filling up the breach or rather rebuilding the funnel: But the landlord rather choſe to ſtop up the chimney.

Another puzzling caſe I met with at a friend's country houſe near London. His beſt room had a chimney in which, he told me, he never could have a fire, for all the ſmoke came out into the room. I flattered myſelf I could eaſily find the cauſe, and preſcribe the cure. I had a fire made

made there, and found it as he ſaid. I opened the door, and perceived it was not want of air. I made a temporary contraction of the opening of the chimney, and found that it was not its being too large that cauſed the ſmoke to iſſue. I went out and looked up at the top of the chimney: Its funnel was joined in the ſame ſtack with others, ſome of them ſhorter, that drew very well, and I ſaw nothing to prevent its doing the ſame. In fine, after every other examination I could think of, I was obliged to own the inſufficiency of my ſkill. But my friend, who made no pretenſion to ſuch kind of knowledge, afterwards diſcovered the cauſe himſelf. He got to the top of the funnel by a ladder, and looking down, found it filled with twiggs and ſtraw cemented by earth, and lined with feathers. It ſeems the houſe, after being built, had ſtood empty ſome years before he occupied it; and he concluded that ſome large birds had taken the advantage of its retired ſituation to make their neſt there. The rubbiſh, conſiderable in quantity, being removed, and the funnel cleared, the chimney drew well, and gave ſatisfaction.

In general, ſmoke is a very tractable thing, eaſily governed and directed when one knows the principles, and is well informed of the circumſtances. You know I made it *deſcend* in my Pennſylvania ſtove. I formerly had a more ſimple conſtruction, in which the ſame effect was produced, but viſible to the eye. It was compoſed of two plates A B and C D, placed as in the figure. The lower plate A B reſted with its edge in the angle made by the hearth with the back of the chimney. The upper plate was fixed to the breaſt, and lapt over the lower about ſix inches, leaving a ſpace of four inches wide and the length of the plates (near two feet) between them. Every other paſſage of air into the funnel was well ſtopped. When therefore a fire was made at E, for the firſt time with charcoal, till the air in the funnel was a little heated through the plates, and then wood

Plate I. Figure 7.

wood laid on, the ſmoke would riſe to A, turn over the edge of that plate, deſcend to D, then turn under the edge of the upper plate, and go up the chimney. It was pretty to ſee, but of no great uſe. Placing therefore the under plate in a higher ſituation, I removed the upper plate C D, and placed it perpendicularly, ſo that the upper edge of the lower plate A B came within about three inches of it, and might be puſhed farther from it, or ſuffered to come nearer to it by a moveable wedge between them. The flame then aſcending from the fire at E, was carried to ſtrike the upper plate, made it very hot, and its heat roſe and ſpread with the rarefied air into the room. Plate I. Figure 8.

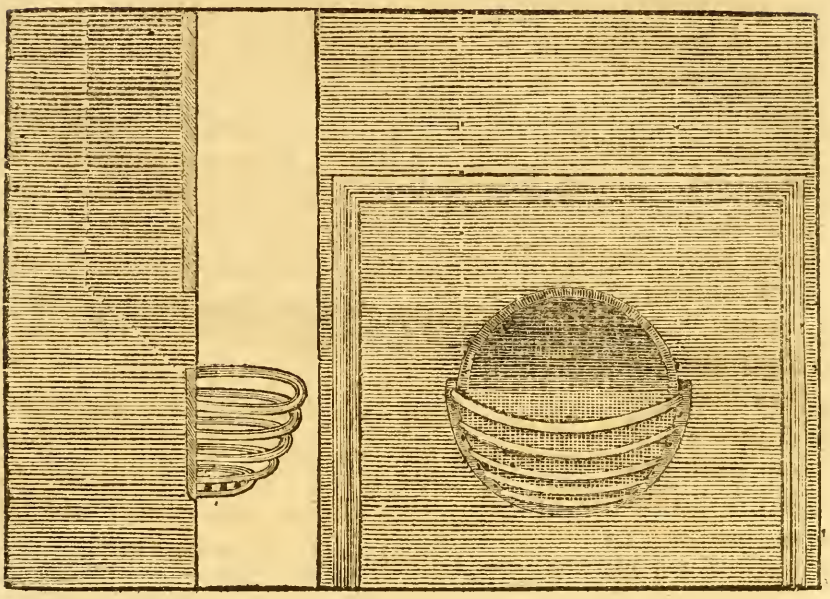
I believe you have ſeen in uſe with me, the contrivance of a ſliding-plate over the fire, ſeemingly placed to oppoſe the riſing of the ſmoke, leaving but a ſmall paſſage for it, between the edge of the plate and the back of the chimney. It is particularly deſcribed, and its uſes explained, in my former printed letter, and I mention it here only as another inſtance of the tractability of ſmoke*.

What is called the Staffordſhire chimney, affords an example of the ſame kind. The opening of the chimney is bricked up, even with the fore-edge of its jams, leaving open only a paſſage over the grate of the ſame width, and perhaps eight inches high. The grate conſiſts of ſemicircular bars, their upper bar of the greateſt diameter, the others under it ſmaller and ſmaller, ſo that it has the appearance of half a round baſket. It is, with the coals it contains, wholly without the wall that ſhuts up the chimney, yet the ſmoke bends and enters the paſſage above it, the draft being ſtrong, becauſe no air can enter that is not obliged to paſs near or through the fire, ſo that all that the funnel is filled with is much heated, and of courſe much rarefied.

* See Appendix, N° II.

STAFFORDSHIRE FIRE-PLACE.

SIDE VIEW. FRONT VIEW.



Much more of the proſperity of a winter country depends on the plenty and cheapneſs of fuel, than is generally imagined. In travelling I have obſerved, that in thoſe parts where the inhabitants can have neither wood nor coal nor turff but at exceſſive prices, the working people live in miſerable hovels, are ragged, and have nothing comfortable about them. But where fuel is cheap, (or where they have the art of managing it to advantage) they are well furniſhed with neceſſaries, and have decent habitations. The obvious reaſon is, that the working hours of ſuch people are the profitable hours, and they who cannot afford ſufficient fuel have fewer ſuch hours in the twenty four, than thoſe who have it cheap and plenty: For much of the domeſtic work of poor women, ſuch as ſpinning,

ſpinning, ſewing, knitting; and of the men in thoſe manufactures that require little bodily exerciſe, cannot well be performed where the fingers are numbed with cold: Thoſe people, therefore, in cold weather, are induced to go to bed ſooner, and lie longer in a morning, than they would do if they could have good fires or warm ſtoves to ſit by; and their hours of work are not ſufficient to produce the means of comfortable ſubſiſtence. Thoſe public works, therefore, ſuch as roads, canals, &c. by which fuel may be brought cheap into ſuch countries from diſtant places, are of great utility; and thoſe who promote them may be reckoned among the benefactors of mankind.

I have great pleaſure in having thus complied with your requeſt, and in the reflection that the friendſhip you honour me with, and in which I have ever been ſo happy, has continued ſo many years without the ſmalleſt interruption. Our diſtance from each other is now augmented, and nature muſt ſoon put an end to the poſſibility of my continuing our correſpondence: But if conſciouſneſs and memory remain in a future ſtate, my eſteem and reſpect for you, my dear friend, will be everlaſting.

B. F.

APPENDIX.

NOTES FOR THE LETTER UPON CHIMNEYS.

N° I.

THE lateſt work on architecture that I have ſeen, is that entitled NUTSHELLS, which appears to be written by a very ingenious man, and contains a table of the proportions of the openings of chimneys; but they relate ſolely to the proportions he gives his rooms, without the ſmalleſt regard to the funnels. And he remarks, reſpecting thoſe proportions, that they are ſimilar to the harmonic diviſions of a monochord*. He does not indeed lay much ſtreſs on this; but it ſhows that we like the appearance of principles; and where we have not true ones, we have ſome ſatisfaction in producing ſuch as are imaginary.

N° II.

THE deſcription of the ſliding plates here promiſed, and which hath been ſince brought into uſe under various names, with ſome immaterial changes, is contained in a former letter to J. B. Eſq. as follows:

To J. B. *Eſq. at Boſton, in New-England.*

Dear Sir, London, Dec. 2, 1758.

I HAVE executed here an eaſy ſimple contrivance, that I have long ſince had in ſpeculation, for keeping rooms warmer

* "It may be juſt remarked here, that upon comparing theſe proportions with thoſe ariſing "from the common diviſions of the monochord, it happens that the firſt anſwers to uniſon, "and although the ſecond is a diſcord, the third anſwers to the third minor, the fourth to the "third major, the fifth to the fourth, the ſixth to the fifth, and the ſeventh to the octave." NUTSHELLS, page 85.

warmer in cold weather than they generally are, and with leſs fire. It is this. The opening of the chimney is contracted, by brick-work faced with marble ſlabs, to about two feet between the jams, and the breaſt brought down to within about three feet of the hearth.—An iron frame is placed juſt under the breaſt, and extending quite to the back of the chimney, ſo that a plate of the ſame metal may ſlide horizontally backwards and forwards in the grooves on each ſide of the frame. This plate is juſt ſo large as to fill the whole ſpace, and ſhut the chimney entirely when thruſt quite in, which is convenient when there is no fire. Drawing it out, ſo as to leave a ſpace between its further edge and the back, of about two inches; this ſpace is ſufficient for the ſmoke to paſs; and ſo large a part of the funnel being ſtopt by the reſt of the plate, the paſſage of warm air out of the room, up the chimney, is obſtructed and retarded, and by that means much cold air is prevented from coming in through crevices, to ſupply its place. This effect is made manifeſt three ways. Firſt, when the fire burns briſkly in cold weather, the howling or whiſtling noiſe made by the wind, as it enters the room through the crevices, when the chimney is open as uſual, ceaſes as ſoon as the plate is ſlid in to its proper diſtance. Secondly, opening the door of the room about half an inch, and holding your hand againſt the opening, near the top of the door, you feel the cold air coming in againſt your hand, but weakly, if the plate be in. Let another perſon ſuddenly draw it out, ſo as to let the air of the room go up the chimney, with its uſual freedom where chimneys are open, and you immediately feel the cold air ruſhing in ſtrongly. Thirdly, if ſomething be ſet againſt the door, juſt ſufficient, when the plate is in, to keep the door nearly ſhut, by reſiſting the preſſure of the air that would force it open: Then, when the plate is drawn out, the door will be forced open by the increaſed preſſure of the outward cold air endeavouring to get in to ſupply the place of the warm

warm air, that now paſſes out of the room to go up the chimney. In our common open chimneys, half the fuel is waſted, and its effect loſt; the air it has warmed being immediately drawn off. Several of my acquaintance having ſeen this ſimple machine in my room, have imitated it at their own houſes, and it ſeems likely to become pretty common. I deſcribe it thus particularly to you, becauſe I think it would be uſeful in *Boſton*, where firing is often dear.

Mentioning chimneys puts me in mind of a property I formerly had occaſion to obſerve in them, which I have not found taken notice of by others; it is, that in the ſummer time, when no fire is made in the chimneys, there is, nevertheleſs, a regular draft of air through them; continually paſſing upwards, from about five or ſix o'clock in the afternoon, till eight or nine o'clock the next morning, when the current begins to ſlacken and heſitate a little, for about half an hour, and then ſets as ſtrongly down again, which it continues to do till towards five in the afternoon, then ſlackens and heſitates as before, going ſometimes a little up, then a little down, till in about a half an hour it gets into a ſteady upward current for the night, which continues till eight or nine the next day; the hours varying a little as the days lengthen and ſhorten, and ſometimes varying from ſudden changes in the weather; as if, after being long warm, it ſhould begin to grow cool about noon, while the air was coming down the chimney, the current will then change earlier than the uſual hour, *&c.*

This property in chimneys I imagine we might turn to ſome account, and render improper, for the future, the old ſaying, *as uſeleſs as a chimney in ſummer.* If the opening of the chimney, from the breaſt down to the hearth, be cloſed by a ſlight moveable frame or two, in the manner of doors, covered with canvas, that will let the air through, but keep out the flies; and another little frame ſet within upon the hearth, with hooks on which to hang joints of meat, fowls, *&c.* wrapt well in wet linen cloths, three or four

four fold, I am confident that if the linen is kept wet, by ſprinkling it once a day, the meat would be ſo cooled by the evaporation, carried on continually by means of the paſſing air, that it would keep a week or more in the hotteſt weather. Butter and milk might likewiſe be kept cool, in veſſels or bottles covered with wet cloths. A ſhallow tray, or keeler, ſhould be under the frame to receive any water that might drip from the wetted cloths. I think, too, that this property of chimneys might, by means of ſmoke-jack vanes, be applied to ſome mechanical purpoſes, where a ſmall but pretty conſtant power only is wanted.

If you would have my opinion of the cauſe of this changing current of air in chimneys, it is, in ſhort, as follows. In ſummer time there is generally a great difference in the warmth of the air at mid-day and midnight, and, of courſe, a difference of ſpecific gravity in the air, as the more it is warmed the more it is rareſied. The funnel of a chimney being for the moſt part ſurrounded by the houſe, is protected, in a great meaſure, from the direct action of the ſun's rays, and alſo from the coldneſs of the night air. It thence preſerves a middle temperature between the heat of the day, and the coldneſs of the night. This middle temperature it communicates to the air contained in it. If the ſtate of the outward air be cooler than that in the funnel of the chimney, it will, by being heavier, force it to riſe, and go out at the top. What ſupplies its place from below, being warmed, in its turn, by the warmer funnel, is likewiſe forced up by the colder and weightier air below, and ſo the current is continued till the next day, when the ſun gradually changes the ſtate of the outward air, makes it firſt as warm as the funnel of the chimney can make it, (when the current begins to heſitate) and afterwards warmer. Then the funnel being cooler than the air that comes into it, cools that air, makes it heavier than the outward air, of courſe it deſcends; and what ſucceeds it from above, being cooled in its turn, the deſcending current continues

tinues till towards evening, when it again hesitates and changes its course, from the change of warmth in the outward air, and the nearly remaining same middle temperature in the funnel.

Upon this principle, if a house were built behind *Beacon-hill*, an adit carried from one of the doors into the hill horizontally, till it met with a perpendicular shaft sunk from its top, it seems probable to me, that those who lived in the house, would constantly, in the heat even of the calmest day, have as much cool air passing through the house, as they should chuse; and the same, though reversed in its current, during the stillest night.

I think, too, this property might be made of use to miners; as where several shafts or pits are sunk perpendicularly into the earth, communicating at bottom by horizontal passages, which is a common case, if a chimney of thirty or forty feet high were built over one of the shafts, or so near the shaft, that the chimney might communicate with the top of the shaft, all air being excluded but what should pass up or down by the shaft, a constant change of air would, by this means, be produced in the passages below, tending to secure the workmen from those damps which so frequently incommode them. For the fresh air would be almost always going down the open shaft, to go up the chimney, or down the chimney to go up the shaft. Let me add one observation more, which is, that if that part of the funnel of a chimney, which appears above the roof of a house, be pretty long, and have three of its sides exposed to the heat of the sun successively, viz. when he is in the east, in the south, and in the west, while the north side is sheltered by the building from the cool northerly winds; such a chimney will often be so heated by the sun, as to continue the draft strongly upwards, through the whole twenty four hours, and often for many days together. If the outside of such a chimney be painted black, the effect will be still greater, and the current stronger.

N° III.

N° III.

IT is ſaid the northern Chineſe have a method of warming their ground floors, which is ingenious. Thoſe floors are made of tile a foot ſquare and two inches thick, their corners being ſupported by bricks ſet on end, that are a foot long and four inches ſquare, the tiles, too, join into each other, by ridges and hollows along their ſides. This forms a hollow under the whole floor, which on one ſide of the houſe has an opening into the air, where a fire is made, and it has a funnel riſing from the other ſide to carry off the ſmoke. The fuel is a ſulphurous pitcoal, the ſmell of which in the room is thus avoided, while the floor and of courſe the room is well warmed. But as the underſide of the floor muſt grow foul with ſoot, and a thick coat of ſoot prevents much of the direct application of the hot air to the tiles, I conceive that burning the ſmoke by obliging it to deſcend through red coals, would in this conſtruction be very advantageous, as more heat would be given by the flame than by the ſmoke, and the floor being thereby kept free from ſoot would be more heated with leſs fire. For this purpoſe I would propoſe erecting the funnel cloſe to the grate, ſo as to have only an iron plate between the fire and the funnel, through which plate the air in the funnel being heated, it will be ſure to draw well, and force the ſmoke to deſcend, as in the figure where A is the funnel or chimney, B the grate on which the fire is placed, C one of the apertures through which the deſcending ſmoke is drawn into the channel D of figure 10, along which channel it is conveyed by a circuitous rout, as deſignated by the arrows, until it arrives at the ſmall aperture E, figure 10, through which it enters the funnel F. G in both figures is the iron plate againſt which the fire is made, which being heated thereby, will rarefy the air in that part of the funnel, and cauſe the ſmoke to aſcend rapidly. The flame thus dividing

Plate I. Figure 9.

from the grate to the right and left, and turning in paſſages diſpoſed, as in figure 13, ſo as that every part of the floor may be viſited by it before it enters the funnel F, by the two paſſages E E, very little of the heat will be loſt, and a winter room thus rendered very comfortable.

N° IV.

PAGE 8. *Few can imagine*, &c. It is ſaid the Icelanders have very little fuel, chiefly drift wood that comes upon their coaſt. To receive more advantage from its heat, they make their doors low, and have a ſtage round the room above the door, like a gallery, wherein the women can ſit and work, the men read or write, &c. The roof being tight, the warm air is confined by it and kept from riſing higher and eſcaping; and the cold air which enters the houſe when the door is opened, cannot riſe above the level of the top of the door, becauſe it is heavier than the warm air above the door, and ſo thoſe in the gallery are not incommoded by it. Some of our too lofty rooms might have a ſtage ſo conſtructed as to make a temporary gallery above, for the winter, to be taken away in ſummer. Sedentary people would find much comfort there in cold weather.

N° V.

PAGE 26. *Where they have the art of managing it*, &c. In ſome houſes of the lower people among the northern nations of Europe, and among the poorer ſort of Germans in Pennſylvania, I have obſerved this conſtruction, which appears very advantageous. A is the kitchen with its chimney; B an iron ſtove in the ſtove-room. In a corner of the chimney is a hole through the back into the ſtove, to put in fuel, and another hole above it to let the ſmoke of the ſtove come back into the chimney. As ſoon as the cooking is over, the brands in the kitchen chimney are put

Plate I. Figure 11.

put through the hole to ſupply the ſtove, ſo that there is ſeldom more than one fire burning at a time. In the floor over the ſtove-room, is a ſmall trap door, to let the warm air riſe occaſionally into the chamber. Thus the whole houſe is warmed at little expence of wood, and the ſtove-room kept conſtantly warm; ſo that in the coldeſt winter nights, they can work late, and find the room ſtill comfortable when they riſe to work early. An Engliſh farmer in America who makes great fires in large open chimneys, needs the conſtant employment of one man to cut and haul wood for ſupplying them; and the draft of cold air to them is ſo ſtrong, that the heels of his family are frozen while they are ſcorching their faces, and the room is never warm, ſo that little ſedentary work can be done by them in winter. The difference in this article alone of œconomy, ſhall, in a courſe of years, enable the German to buy out the Engliſhman, and take poſſeſſion of his plantation.

MISCELLANEOUS OBSERVATIONS.

CHIMNEYS whoſe funnels go up in the north wall of a houſe and are expoſed to the north winds, are not ſo apt to draw well as thoſe in a ſouth wall; becauſe when rendered cold by thoſe winds, they draw downwards.

Chimneys encloſed in the body of a houſe are better than thoſe whoſe funnels are expoſed in cold walls.

Chimneys in ſtacks are apt to draw better than ſeparate funnels, becauſe the funnels that have conſtant fires in them, warm the others in ſome degree that have none.

One of the funnels in a houſe I once occupied, had a particular funnel joined to the ſouth ſide of the ſtack, ſo that three of its ſides were expoſed to the ſun in the courſe of the day, viz. the eaſt ſide E during the morning, the ſouth ſide S in the middle part of the day, and the weſt ſide W during the afternoon, while its north ſide was ſhelter-

Plate I. Figure 12.

ed by the ſtack from the cold winds. This funnel, which came from the ground floor, and had a conſiderable height above the roof, was conſtantly in a ſtrong drawing ſtate day and night, winter and ſummer.

Blacking of funnels expoſed to the ſun, would probably make them draw ſtill ſtronger.

In Paris I ſaw a fire-place ſo ingeniouſly contrived as to ſerve conveniently two rooms, a bedchamber and a ſtudy. The funnel over the fire was round. The fire-place was of caſt iron, having an upright back A, and two horizontal ſemicircular plates B C, the whole ſo ordered as to turn on the pivots D E. The plate B always ſtopped that part of the round funnel that was next to the room without fire, while the other half of the funnel over the fire was always open. By this means a ſervant in the morning could make a fire on the hearth C, then in the ſtudy, without diſturbing the maſter by going into his chamber; and the maſter when he roſe, could with a touch of his foot turn the chimney on its pivots, and bring the fire into his chamber, keep it there as long as he wanted it, and turn it again when he went out into his ſtudy. The room which had no fire in it, was alſo warmed by the heat coming through the back plate, and ſpreading in the room as it could not go up the chimney.

Plate I. Figure 13.

Explanation

N° II.

Explanation of an Optical Deception.

BY D. RITTENHOUSE.

Read March 3, 1780.

SOME experiments were long ago communicated to the Royal Society of London, ſhewing, that through the double microſcope, the ſurfaces of bodies ſometimes appear to be reverſed, that is, thoſe parts which are elevated ſeem depreſſed, and the contrary. But the cauſe of this appearance, for any thing I know, remains ſtill to be explained.

In order to produce this effect, no other apparatus is neceſſary than two convex lenſes placed in a tube, at a diſtance from each other nearly equal to the ſum of their focal diſtances. Through theſe glaſſes, objects that appear diſtinctly, always appear inverted; for they are not ſeen directly, but by means of an image formed either between the two glaſſes, or between both of them and the eye.

If we look through ſuch glaſſes at corniſhes, picture frames and other mouldings in carpenters work, and ſome ſorts of carved work, thoſe parts which are raiſed generally appear depreſſed, and thoſe parts which are depreſſed appear raiſed. But a very ready object, and which ſucceeds as well as any thing I know of, is a brick pavement; whether it be a chimney-hearth, or pavement out of doors. Viewed through the tube above deſcribed, every little cavity in the bricks, and the chinks between them, almoſt always appear to be ſo many elevations above the ſurface of the bricks.

When I conſidered this odd appearance, the firſt probable cauſe that offered was, that thoſe parts of the object which are ſunk, and fartheſt from the eye, might have their correſpondent parts of the image formed by the glaſſes neareſt to the eye, and therefore would appear raiſed.

But

But this is not the caſe; for thoſe parts which are fartheſt from the eye in the object, will always be fartheſt from the eye in the image, and often in a much greater proportion. After ſome time I concluded it to be a neceſſary conſequence of the apparent inverſion of the object; and many things tended to confirm me in this opinion, before I made the experiments which ſeem perfectly deciſive.

It has often been matter of ſurprize to me, when viewing the moon through a good teleſcope, in company with perſons not accuſtomed to ſuch obſervations, that whilſt the cavities and eminences of the moon's ſurface appeared to me marked out with the utmoſt certainty by their light and ſhades, my companions generally conceived it to be a plain ſurface of various degrees of brightneſs. The reaſon I ſuppoſe to be this; the aſtronomer knows from the moon's ſituation with reſpect to the ſun, and even from the figure of its enlightened part, preciſely in what direction the light falls on its ſurface, and therefore judges rightly of its hills and vallies, from their different degrees of light, according to thoſe rules which are imperceptibly formed in the mind, and confirmed by long experience. But a perſon unacquainted with aſtronomy knows nothing of the direction of the ſun's light on the moon, nor does he attend to the moon's globular figure, and is beſides, perhaps, poſſeſſed with a notion of its being ſelf-luminous; no wonder then that the ſame object has a very different effect on his imagination. It ſeems to be thoſe rules of judging, which we begin to form in our earlieſt infancy, which we ſet aſide, re-eſtabliſh, alter, correct and confirm, and at length rely on with the utmoſt confidence, even without knowing that we do ſo, or that we have any ſuch rules: It is theſe rules, of ſuch infinite general uſe to us, that ſometimes miſlead us on new and extraordinary occaſions, and particularly in the caſe now before us. A perſon entering into a room perceives, at a ſingle glance, whence the light comes which illuminates the objects before

fore him; and that without remaining conſcious for a moment that he has attended to this circumſtance: But the effect remains, and will influence his judgment. If on looking at a brick hearth he perceives that thoſe lines which divide the bricks have a dark ſhade on that ſide oppoſite to the light, and a bright ſtreak on the contrary ſide next to the light, he muſt at the ſame time perceive that they have the property which he has conſtantly obſerved in ridges, not in furrows. And ſince the appearance of the hearth will be ſuch, through the glaſſes, in conſequence of their inverting the ſituation of its ſeveral parts, with reſpect to the light, the obſerver will inſtantly pronounce the chinks between the bricks, and every little cavity in them, to be ſo many perfect elevations above the common ſurface, nor can any effort of the mind correct the imagination or alter the appearance.

Though I was well ſatisfied of the truth of this explanation, I reſolved nevertheleſs to bring it to the teſt of experiment, which I did in the following manner.

In order to give my experiment fair play, I ſhut all the windows of my chamber excepting one directly oppoſite to the chimney. I then took the tube, with two convex glaſſes, and looking through it at the hearth, all the bricks appeared depreſſed and the clefts between them elevated, as uſual. I then placed a looking-glaſs againſt the chimney back, ſo that it reflected the light from the window upon the hearth, and ſet up a ſmall board before the hearth to intercept the direct light of the window from it. Then looking at the hearth through the glaſſes, I was much pleaſed to find it appear in its natural ſtate, with the bricks elevated. I then ſat down on a chair at the edge of the hearth, and looking through the tube which I held to my eye with one hand, whilſt with the other I moved the board ſo as to make it ſometimes intercept the direct light of the window, and at other times the reflected light of the looking-glaſs, I conſtantly found that when the hearth

was

was illuminated by reflected light, it appeared in its natural ſtate, and when illuminated by the direct light, in its unnatural ſtate; for ſo I call it when the bricks appear depreſſed and the chinks between them elevated.

I then conſidered that ſince the hearth appeared in its natural ſtate by reflected light, and in its unnatural ſtate by direct light, only in conſequence of the inverting property of the glaſſes, the appearance ought to be directly the contrary when it was viewed with the naked eye. And accordingly I found, upon taking out both of the glaſſes, and looking through the open tube, that the hearth appeared as perfectly, and as conſtantly in its unnatural ſtate by reflected light, and in its natural ſtate by direct light, as it had before done the reverſe through the glaſſes. But it muſt be obſerved that ſomething like a tube is neceſſary to confine the ſight from other adjoining objects, which not being in the ſame circumſtances would otherwiſe correct the imagination.

If we look through ſuch a tube and glaſſes at the hearth or other object, ſuppoſe a piece of chocolate, the furrows in it appear ſo many ridges, on removing the tube they ſink into furrows, on applying it they again riſe into ridges, and the illuſion might perhaps be repeated a thouſand times, without the mind being at all able to conceive the object to appear through the tube like what it really is. But if whilſt you are looking through the tube, and the object appears in its unnatural ſtate, that is, when its furrows appear ridges, you apply your finger and feel that they really are furrows, the deception vaniſhes in a moment and the object appears in its natural ſtate. This I at firſt ſuppoſed to ariſe from the ſuperior confidence which we have in the ſenſe of touching, as knowing by experience that this ſenſe more perfectly repreſents the figure of bodies than the ſight does. But I was, at leaſt in part, miſtaken. For if whilſt you ſee the object in its unnatural ſtate, another perſon puts his finger to the part you are looking

looking at, the deception vaniſhes as well as in the former caſe. The application of a writing pen or pencil will produce the ſame effect. And, which is very remarkable, after the mind has been undeceived by theſe means once or twice, it does not readily admit of the impoſition again: Though, as I obſerved before, if it be done by removing the glaſſes, the deception will return again as often as you pleaſe. The truth ſeems to be, that the mind chuſes the leaſt difficulty; and though in conſequence of the judgment it has formed concerning the direction of the light, it will ſubmit to ſuch a ſmall impoſition as to ſuppoſe one piece of chocolate may have ridges where others uſually have furrows, when indeed it has not, yet it will not readily endure ſuch a groſs one, as to ſuppoſe it to have cavities of the figure and colour of a finger or a writing pen. Or perhaps the viſible motion attending ſuch application produces the principal effect in convincing the mind that thoſe bodies are really elevated*, and then their ſhades and modifications of the light, ſhew in what direction it falls on them; and the miſtake of the mind in that particular being rectified, the whole object muſt aſſume its natural appearance.

The explanation I have given of this phænomenon will account for an odd circumſtance mentioned (I think) by Mr. Short; which once appeared ſo whimſical to me as neither to merit credit or attention. Mr. Short carefully examined the Caſſegrain teleſcope, and in all probability ſet it by the ſide of one of the Gregorian form, in order to determine its comparative merits: He gives the preference to the Gregorian, and mentions as a principal defect of the Caſſegrain teleſcope, that it repreſents the mountains in the moon as vallies, and the contrary. I doubt not but this,

* Whilſt I was making theſe experiments, I thought of a carved ſilver ſhoe buckle, as a very proper object to prevent a deception of this ſort from taking place. But placing it on a brick pavement, and looking at it through the glaſſes, it nevertheleſs appeared perfectly depreſſed. Preciſely as if you had taken a buckle and ſtrewing on it a white ſhining powder, had preſſed it into the brick whilſt ſoft, and then removing the buckle, the glittering powder had remained in the impreſſion.

otherwiſe unaccountable appearance, was occaſioned intirely by its inverting the object, for the reaſons above given. If it be aſked, why then do not the common long refractors, which generally invert, produce the ſame deception? I anſwer, very probably they would do ſo if ſet beſide a Gregorian reflector and the eye applied alternately to the one and to the other*.

N° III.

Deſcription of the White Mountains in New-Hampſhire.

By the Rev. JEREMY BELKNAP of New-Hampshire.

Read Oct. 15, 1784.

THE white mountains in the northern part of New-Hampſhire have, from the earlieſt ſettlement of the country, attracted the attention of all ſorts of perſons. They are undoubtedly the higheſt lands in New-England, and are diſcovered in clear weather by veſſels coming on the eaſtern coaſt, before any other land; but by reaſon of their bright appearance are frequently miſtaken for clouds. They are ſeen on ſhore at the diſtance of ſixty or eighty miles on the ſouth and ſouth-eaſt ſides, and are ſaid to be plainly viſible in the neighbourhood of Quebec. The Indians had a ſuperſtitious veneration for them as the habitation of inviſible beings, and for this reaſon never ventured to aſcend their ſummits, and always endeavoured to diſcourage every perſon who attempted it. From them, and the captives whom they formerly led to Canada through the paſs of theſe mountains, many fictions have been propagated through the country which have in time ſwelled to marvellous and incredible ſtories; particularly,

* The above was written in 1774, when I had no achromatic aſtronomical teleſcope in my poſſeſſion.

larly, it has been reported that carbuncles have been ſeen at immenſe heights, and inacceſſible ſpots which give a luſtre in the night.

Thoſe who have attempted to give an account of theſe mountains, have aſcribed their brightneſs to ſhining rocks or white moſs, and the higheſt ſummit has been repreſented as inacceſſible by reaſon of the extreme cold which threatens to freeze the traveller in the midſt of ſummer. They have alſo differed ſo widely from each other, and their accounts have been embelliſhed with ſo many marvellous circumſtances, and on the whole have been ſo unſatisfactory, that I have long wiſhed for an opportunity to viſit theſe mountains in company with ſome gentlemen of a philoſophical turn, furniſhed with proper inſtruments and materials for a full exploration of the phenomena that might occur. This pleaſure I have in part enjoyed the preſent ſummer; and though the roughneſs of the way which prevented the uſe of convenient carriages, proved fatal to ſome of our inſtruments, and the almoſt continual cloudineſs of the weather while we were in that region hindered us from making ſome obſervations which we intended; yet till a better account can be obtained, I flatter myſelf that what follows will prove more ſatisfactory than any which has yet been publiſhed or reported.

The white mountains are the higheſt part of a ridge which extends north-eaſt and ſouth-weſt to an unknown length. The area of their baſe is an irregular figure ſomewhat reſembling an iſoceles triangle, whoſe longeſt extremity is toward the ſouth, and whoſe whole circuit cannot be leſs than fifty miles. The number of ſummits within this area cannot be aſcertained at preſent, the country round them being a thick wilderneſs. On the northweſt ſide ſeven ſummits are in plain view, and this is the greateſt number that can be ſeen at once from any ſtation that is cleared of woods. Of theſe, four at leaſt are bald. The higheſt of them is on the eaſtern ſide of the cluſter,

on which ſide we aſcended, having firſt gained the height of land between the waters of Saco and Amariſcogin rivers, to which there is a gradual aſcent for twelve miles from the plains of Pigwacket. At this height of land there is a meadow which was formerly a beaver-pond with a dam at each end. The water iſſues out of a mountain on its eaſtern ſide in the form of ſprings, and meandering through the channels of the meadow appears ſtagnant in the middle but dividing its courſe, at the ſouth end of the meadow it runs into Ellis river, a branch of Saco; and at the north end into Peabody river, a branch of Amariſcogin. From this meadow there is an uninterrupted aſcent on a ridge between two deep gullies to the higheſt ſummit.

The ſides of the mountains are covered with ſpruce trees; the ſurface is compoſed of looſe rocks covered with very long green moſs, which reaches from rock to rock, and is in many places ſo thick and ſtrong as to ſupport a man's weight. This immenſe bed of moſs, ſpread over the ſurface of theſe mountains ſerves, as a ſponge to retain the moiſture brought by the clouds and vapors which are continually riſing and gathering round the mountains; the thick growth of ſpruce prevents the ſun's rays from penetrating to exhale it; ſo that there is a conſtant ſupply of water to the numberleſs ſprings with which this region abounds, and an unceaſing circulation of fluid, the proceſs of which is highly entertaining to the ſpectator; for no ſooner has a ſhower deſcended from the clouds, but the vapor riſes from the leaves of the foreſt in innumerable little columns, which, having gained a certain height in the atmoſphere, collect and converge toward the mountains, where they either fall again in ſhowers or are imbibed by the moſs and depoſited in the crevices of the rocks, ſeeking their way to the hard ſtratum or pan which is impenetrable, and which guides them till they find vent in ſprings. The ſame liquid tribute is daily exhaled from the rivers, ponds and low grounds, and attracted to the mountains,

mountains, which by theſe means are always repleniſhed with water in every part.

The rocks, of which theſe mountains are compoſed, are in ſome parts ſlate, in others flint, but toward the top a dark grey ſtone, which, when broken, ſhows ſpecks of iſing-glaſs. On the bald parts of the mountains the ſtones are covered with a ſhort grey moſs, and at the very ſummit the moſs is of a yellowiſh colour and adheres firmly to the rock.

Eight of our company aſcended the higheſt mountain on the 24th of July, and were ſix hours and fifty one minutes in gaining the ſummit, deducting one hour and thirty eight minutes for the neceſſary ſtops. The ſpruce and firs, as you aſcend, grow ſhorter till they degenerate to ſhrubs and buſhes, then you meet with low vines bearing a red and a blue berry, and laſtly a ſort of graſs called winter graſs mixed with the moſs.

Having aſcended the ſteepeſt precipice, you come to what is called the plain, where the aſcent becomes gentle and eaſy. This plain is compoſed of rocks, covered with winter graſs and moſs, and looks like the ſurface of a dry paſture or common. In ſome openings between the rocks you meet with water, in others dry gravel. The plain is an irregular figure, its area uncertain, but from its eaſtern edge to the foot of the ſugar-loaf, is upwards of a mile; on the weſtern ſide it extends farther. The ſugar-loaf is a pyramidal heap of looſe grey rocks, not leſs than three hundred feet in perpendicular height, but the aſcent is not ſo difficult as the precipice below the plain. From this ſummit in clear weather is a noble view, extending to the ocean on the ſouth-eaſt; to the highlands on the weſt and north-weſt, which ſeparate the waters of Connecticut river from thoſe of lake Champlain and St. Laurence; on the ſouth it extends to Winipiſeogee lake, and the highlands ſouthward of Pemigewaſſet river.

It

It happened unfortunately for our company, that a thick cloud covered the mountain almoſt the whole time that they were on it, ſo that ſome of the inſtruments which, with much labor they had carried up, were uſeleſs. In the barometer the mercury ranged at 22.6 inches, in 44 degrees of heat by Fahrenheit's thermometer. It was our intention to have placed one of each of theſe inſtruments at the foot of the mountain, at the ſame time that others were carried to the top; but they were unhappily broken in the courſe of our journey, and the barometer which was carried to the ſummit, had ſuffered ſo much agitation that an allowance was neceſſary to be made in calculating the height of the mountain, which our ingenious companion, the Rev. Mr. Cutler, of Ipſwich, eſtimates in round numbers at 5500 feet above the meadow, the meadow being 3500 feet above the level of the ſea, and this ſeems to be as low an eſtimation as can be admitted. We intended to have made a geometrical menſuration of the altitude, but in one place where we attempted it, we could not obtain a baſe of ſufficient length, and in another, where this convenience was removed, we were prevented by the almoſt continual obſcurations of the mountains by clouds.

On every ſide of theſe mountains are many long winding gullies, beginning at the precipice below the plain and deepening in the deſcent; they are from one hundred to one thouſand feet deep, and perhaps more. In winter, the ſnow driving with the north-weſt winds over the tops of the mountains, is lodged in theſe gullies, and forms a compact body which is not eaſily diſſolved by the vernal ſun. It is obſerved to lie longer on the ſouth, than on the north-weſt ſides; which is the caſe with moſt other hills in this part of the country. In 1774 ſome men who were at work on a road under the eaſtern ſide of the mountain, aſcended to the ſummit on the 6th of June, and upon the ſouth ſide found a body of ſnow thirteen feet deep, and ſo hard as to bear

bear them. The man from whom I had this account, and who had the direction of the work, aſcended the mountain on the 19th of June, with ſome of the ſame party, and in the ſame ſpot the ſnow was five feet deep. On the 22d of July this year, we were aſſured by perſons who live within plain view of the mountains, on the ſouth ſide, at the diſtance of ſixteen miles, that the ſnow had not been gone more than ten days. We were alſo credibly informed that two men, who attempted to aſcend the mountain the firſt week of September laſt year, found the bald top ſo covered with ſnow and ice, then newly made, that they could not gain the ſummit; but this does not happen every year ſo ſoon, for the mountain has been aſcended ſo late as the firſt week in October, when no ſnow was upon it; and ſometimes the firſt ſnows that come diſſolve before the winter ſets in; but generally the mountains begin to be covered with ſnow and ice, either in the latter part of September, or the beginning of October, and it never wholly leaves them till July. During this period of nine or ten months, they exhibit more or leſs of that bright appearance, from which they are denominated *white*. In the ſpring when the ſnow is partly diſſolved, they appear of a pale blue ſtreaked with white; and after it is wholly gone, at the diſtance of forty or ſixty miles, they are altogether of a pale blue inclining to the colour of the ſky; while viewed at the diſtance of only ten miles, they are of the grey colour of the rock inclining to brown. Theſe changes are obſerved by people who live within conſtant view of them, and from theſe facts and obſervations it may juſtly be concluded that the whiteneſs of them is to be aſcribed wholly to the ſnow and ice and not to any other white ſubſtance, for in reality there is none. There are indeed in the ſummer months ſome ſtreaks which appear brighter than other parts, but theſe, when viewed through a teleſcope, I have plainly diſcerned to be the enlightened edges or ſides of the long deep gullies, and the dark parts the ſhaded ſides

of

of them; and in the courſe of a day theſe ſpots may be ſeen to vary according to the poſition of the ſun.

It may not be amiſs to query here, if ſo great a quantity of ſnow is accumulated and remains on theſe mountains, may it not be ſuppoſed to add a keenneſs to the winds which blow over them? And how many more mountains may there be toward the north and weſt, whoſe hoary ſummits contain the like or greater bodies of ſnow and ice, ſome of which, at the remoteſt regions, may remain undiſſolved through the year? May we not then aſcribe the piercing cold of our north-weſt winds to the infinite ranges of frozen mountains, rather than to the lakes and foreſts?

Theſe immenſe heights which I have been deſcribing, being copiouſly repleniſhed with water, exhibit a variety of beautiful caſcades, ſome of which fall in a perpendicular ſheet or ſpout, others are winding and narrow, others ſpread on the level ſurface of ſome wide rock and then guſh in cataracts over its edge. A romantic imagination may find full gratification amidſt theſe rugged ſcenes, if its ardor be not checked by the fatigue of the approach. Three of the largeſt rivers in New-England receive a great part of their waters from this region. Amonooſuck and Iſrael rivers, two principal branches of Connecticut, fall from the weſtern ſide of the mountains, Peabody river and another branch of Amariſcogin from the north-eaſtern ſide, and almoſt the whole of Saco deſcends from the ſouthern ſide. The declivities being very ſteep cauſe this latter river to riſe very ſuddenly in a time of rain, and as ſuddenly to ſubſide.

On the weſtern part of theſe mountains is a paſs which in the narroweſt place meaſures but twenty two feet between two perpendicular rocks. Here a road is conſtructing with great labor and expence, which is the ſhorteſt rout to the upper Cohoſs on Connecticut river, and to that part of Canada which borders on the river St. Francis. At the height of this narrow paſs the river Saco takes its riſe.

A brook

A brook deſcends from the mountain, and meanders through a meadow which was formerly a beaver-pond, and is ſurrounded by ſteep, and on one ſide, perpendicular rocks—a ſtrikingly pictureſque ſcene! the rivulet glides along the weſtern ſide of the defile, (the eaſtern being formed into a road) and tributary ſtreams augment its waters, one of which is called the Flume, from the near reſemblance it bears to the flume of a mill. The paſs between the mountains widens as you deſcend; but for eight or ten miles they are ſo near as only to leave room for the river and its intervals. In the courſe of this deſcent you ſee at immenſe heights, and in ſpots perfectly inacceſſible, ſeveral rocks, ſome of a whitiſh and ſome of a reddiſh hue, whoſe faces are poliſhed by the continual trickling of water over them. Theſe, when incruſted with ice, being open to the ſouth and weſt, are capable in the night of reflecting the moon and ſtar-beams to the wondering traveller, buried in the dark valley below; and theſe are ſufficient, by the help of imagination, to give riſe to the fiction of carbuncles.

We found no ſtones of any higher quality than flint*; no limeſtone, though we tried the moſt likely with aqua fortis. It is ſaid there is a part of the mountain where the magnetic needle refuſes to traverſe; this may contain rock ore, but our guide could not find the place. It is alſo ſaid that a mineral, ſuppoſed to be lead ore, has been diſcovered on the eaſtern ſide. One of the ſprings which we met with in our aſcent on that ſide afforded a thick frothy ſcum and a ſaponaceous taſte. All ſearches for ſubterranean treaſures in theſe mountains have as yet proved fruitleſs. The moſt certain riches which they yield are the freſhets which bring down the ſoil to the intervals below, and form a fine mould, producing corn, grain and herbage in the moſt luxuriant plenty.

* Some ſpecimens of rock-chryſtal have been found lately by other perſons, but we did not hear of it till after our return.

N° IV.

Deſcription of a remarkable ROCK *and* CASCADE, *near the weſtern ſide of the Youghiogeny river, a quarter of a mile from Crawford's ferry, and about twelve miles from Union-Town, in Fayette county, in the ſtate of Pennſylvania.*

BY THO. HUTCHINS.

Read January 28, 1786.

THIS caſcade is occaſioned by a rock of a ſemicircular form, the chord of which, from one extreme end of the arch to the other, is nearly one hundred yards; the arch or circular part is extenſive, and upwards of twenty feet in height, exhibiting a grand and romantic appearance. This very curious production is compoſed of ſtone of variegated colours, and a ſpecies of marble beautifully chequered with veins running in different directions, preſenting on a cloſe inſpection a faint reſemblance of a variety of mathematical figures of different angles and magnitudes. The operations of nature in this ſtructure ſeems to be exceedingly uniform and majeſtic; the layers or rows of ſtone of which it is compoſed are of various lengths and thickneſſes, more reſembling the effects of art than nature. A flat thin ſtone from eight to ten inches thick, about twenty feet wide, forms the upper part of this amphitheatre, over which the ſtream precipitates. The whole front of this rock is made up from top to bottom, as well as from one extremity of the arch to the other, of a regular ſucceſſion, principally, of limeſtone, ſtrata over ſtrata, and each ſtratum or row, projecting in an horizontal direction a little further out than its baſe, until it terminates into one entire flat, thin, extenſive piece, as already mentioned; and which jets out at right angles

or

or in a parallel line with the bottom, over which it impends fifteen or twenty feet, and that without columns or even a ſingle pillar for its ſupport. This circumſtance, together with the grand circular walk between the front of the rock and the ſheet of water falling from the ſummit, exhibits ſo noble and ſingular an appearance, that a ſpectator cannot behold it without admiration and delight.

N° V.

Letter to Mr. NAIRNE, *of London.*

Paſſy, near Paris, Nov. 13th, 1780.

SIR,

Read January 28, 1786. THE qualities hitherto ſought in a hygrometer, or inſtrument to diſcover the degrees of moiſture and dryneſs in the air, ſeem to have been, an aptitude to receive humidity readily from a moiſt air, and to part with it as readily to a dry air. Different ſubſtances have been found to poſſeſs more or leſs of this quality; but when we ſhall have found the ſubſtance that has it in the greateſt perfection, there will ſtill remain ſome uncertainty in the concluſions to be drawn from the degree ſhown by the inſtrument, ariſing from the actual ſtate of the inſtrument itſelf as to heat and cold. Thus, if two bottles or veſſels of glaſs or metal being filled, the one with cold and the other with hot water, are brought into a room, the moiſture of the air in the room will attach itſelf in quantities to the ſurface of the cold veſſel, while if you actually wet the ſurface of the hot veſſel, the moiſture will immediately quit it, and be abſorbed by the ſame air. And thus in a ſudden change of the air from cold to warm, the inſtrument remaining longer cold may condenſe and abſorb more moiſture, and mark the air as having become

more humid than it is in reality, and the contrary in a change from warm to cold.

But if such a suddenly changing instrument could be freed from these imperfections, yet when the design is to discover the different degrees of humidity in the air of different countries, I apprehend the quick sensibility of the instrument to be rather a disadvantage; since, to draw the desired conclusions from it, a constant and frequent observation day and night in each country will be necessary for a year or years, and the mean of each different set of observations is to be found and determined. After all which some uncertainty will remain respecting the different degrees of exactitude with which different persons may have made and taken notes of their observations.

For these reasons, I apprehend that a substance which, though capable of being distended by moisture and contracted by dryness, is so slow in receiving and parting with its humidity that the frequent changes in the atmosphere have not time to affect it sensibly, and which therefore should gradually take nearly the medium of all those changes and preserve it constantly, would be the most proper substance of which to make such an hygrometer.

Such an instrument, you, my dear sir, though without intending it, have made for me; and I, without desiring or expecting it, have received from you. It is therefore with propriety that I address to you the following account of it; and the more, as you have both a head to contrive and a hand to execute the means of perfecting it. And I do this with greater pleasure, as it affords me the opportunity of renewing that antient correspondence and acquaintance with you, which to me was always so pleasing and so instructive.

You may possibly remember, that in or about the year 1758, you made for me a set of artificial magnets, six in number, each five and a half inches long, half an inch broad, and one eighth of an inch thick. These, with two pieces

pieces of ſoft iron, which together equalled one of the magnets, were incloſed in a little box of mahogany wood, the grain of which ran with, and not acroſs, the length of the box; and the box was cloſed by a little ſhutter of the ſame wood, the grain of which ran acroſs the box; and the ends of this ſhutting piece were bevelled ſo as to fit and ſlide in a kind of dovetail groove when the box was to be ſhut or opened.

I had been of opinion that good mahogany wood was not affected by moiſture ſo as to change its dimenſions, and that it was always to be found as the tools of the workman left it. Indeed the difference at different times in the ſame country, is ſo ſmall as to be ſcarcely in a common way obſervable. Hence the box which was made ſo as to allow ſufficient room for the magnets to ſlide out and in freely, and, when in, afforded them ſo much play that by ſhaking the box one could make them ſtrike the oppoſite ſides alternately, continued in the ſame ſtate all the time I remained in England, which was four years, without any apparent alteration. I left England in Auguſt 1762, and arrived at Philadelphia in October the ſame year. In a few weeks after my arrival, being deſirous of ſhowing your magnets to a philoſophical friend, I found them ſo tight in the box, that it was with difficulty I got them out; and conſtantly during the two years I remained there, viz. till November 1764, this difficulty of getting them out and in continued. The little ſhutter too, as wood does not ſhrink length ways of the grain, was found too long to enter its grooves, and not being uſed, was miſlaid and loſt; and I afterwards had another made that fitted.

In December 1764 I returned to England, and after ſome time I obſerved that my box was become full big enough for my magnets, and too wide for my new ſhutter; which was ſo much too ſhort for its grooves, that it was apt to fall

fall out; and to make it keep in, I lengthened it by adding to each end a little coat of ſealing-wax.

I continued in England more than ten years, and during all that time after the firſt change, I perceived no alteration. The magnets had the ſame freedom in their box, and the little ſhutter continued with the added ſealing-wax to fit its grooves, till ſome weeks after my ſecond return to America.

As I could not imagine any other cauſe for this change of dimenſions in the box, when in the different countries, I concluded, firſt generally that the air of England was moiſter than that of America. And this I ſuppoſed an effect of its being an iſland, where every wind that blew muſt neceſſarily paſs over ſome ſea before it arrived, and of courſe lick up ſome vapour. I afterwards indeed doubted whether it might be juſt only ſo far as related to the city of London, where I reſided; becauſe there are many cauſes of moiſture in the city air, which do not exiſt to the ſame degree in the country; ſuch as the brewers and dyers boiling caldrons, and the great number of pots and teakettles continually on the fire, ſending fourth abundance of vapour; and alſo the number of animals who by their breath continually increaſe it; to which may be added, that even the vaſt quantity of ſea coals burnt there, do in kindling diſcharge a great deal of moiſture.

When I was in England, the laſt time, you alſo made for me a little achromatic pocket teleſcope, the body was braſs, and it had a round caſe, (I think of thin wood) covered with ſhagrin. All the while I remained in England, though poſſibly there might be ſome ſmall changes in the dimenſions of this caſe, I neither perceived nor ſuſpected any. There was always comfortable room for the teleſcope to ſlip in and out. But ſoon after I arrived in America, which was in May 1775, the caſe became too ſmall for the inſtrument, it was with much difficulty and various contrivances that I got it out, and I could never after get

get it in again, during my ſtay there, which was eighteen months. I brought it with me to Europe, but left the caſe as uſeleſs, imagining that I ſhould find the continental air of France as dry as that of Pennſylvania, where my magnet box had alſo returned a ſecond time to its narrowneſs, and pinched the pieces, as heretofore, obliging me too, to ſcrape the ſealing-wax off the ends of the ſhutter.

I had not been long in France, before I was ſurpriſed to find, that my box was become as large as it had always been in England, the magnets entered and came out with the ſame freedom, and, when in, I could rattle them againſt its ſides; this has continued to be the caſe without ſenſible variation. My habitation is out of Paris diſtant almoſt a league, ſo that the moiſt air of the city cannot be ſuppoſed to have much effect upon the box. I am on a high dry hill in a free air as likely to be dry as any air in France. Whence it ſeems probable that the air of England in general may as well as that of London, be moiſter than the air of America, ſince that of France is ſo, and in a part ſo diſtant from the ſea.

The greater dryneſs of the air in America appears from ſome other obſervations. The cabinet work formerly ſent us from London, which conſiſted in thin plates of fine wood glued upon fir, never would ſtand with us, the vaneering, as thoſe plates are called, would get looſe and come off; both woods ſhrinking, and their grains often croſſing, they were forever cracking and flying. And in my electrical experiments there, it was remarkable, that a mahogany table on which my jars ſtood under the prime conductor to be charged, would often be ſo dry, particularly when the wind had been ſome time at north-weſt which with us is a very drying wind, as to iſolate the jars, and prevent their being charged till I had formed a communication between their coatings and the earth. I had a like table in London which I uſed for the ſame purpoſe all the time I reſided there; but it was never ſo dry as to refuſe conducting the electricity. Now

Now what I would beg leave to recommend to you, is, that you would recollect, if you can, the ſpecies of mahogany of which you made my box, for you know there is a good deal of difference in woods that go under that name; or if that cannot be, that you would take a number of pieces of the cloſeſt and fineſt grained mahogany that you can meet with, plane them to the thinneſs of about a line, and the width of about two inches acroſs the grain, and fix each of the pieces in ſome inſtrument that you can contrive, which will permit them to contract and dilate, and will ſhow, in ſenſible degrees, by a moveable hand upon a marked ſcale, the otherwiſe leſs ſenſible quantities of ſuch contraction and dilatation. If theſe inſtruments are all kept in the ſame place while making, and are graduated together while ſubject to the ſame degrees of moiſture or dryneſs, I apprehend you will have ſo many comparable hygrometers, which being ſent into different countries, and continued there for ſome time, will find and ſhow there the mean of the different dryneſs and moiſture of the air of thoſe countries, and that with much leſs trouble than by any hygrometer hitherto in uſe.

With great eſteem,

I am, dear ſir,

Your moſt obedient,

And moſt humble ſervant,

B. FRANKLIN.

Deſcription

N° VI.

Deſcription of a new STOVE *for burning of Pitcoal, and conſuming all its Smoke.*

BY DR. B. FRANKLIN.

Read January 28, 1786.

TOWARDS the end of the laſt century an ingenious French philoſopher, whoſe name I am ſorry I cannot recollect, exhibited an experiment to ſhow that very offenſive things might be burnt in the middle of a chamber, ſuch as woollen rags, feathers, &c. without creating the leaſt ſmoke or ſmell. The machine in which the experiment was made, if I remember right, was of this form, made of plate iron. Some clear burning charcoals were put into the opening of the ſhort tube A, and ſupported there by the grate B. The air as ſoon as the tubes grew warm would aſcend in the longer leg C and go out at D, conſequently air muſt enter at A deſcending to B. In this courſe it muſt be heated by the burning coals through which it paſſed, and riſe more forcibly in the longer tube in proportion to its degree of heat or rarefaction, and length of that tube. For ſuch a machine is a kind of inverted ſyphon; and as the greater weight of water in the longer leg of a common ſyphon in deſcending is accompanied by an aſcent of the ſame fluid in the ſhorter; ſo, in this inverted ſyphon, the greater quantity of levity of air in the longer leg, in riſing is accompanied by the deſcent of air in the ſhorter. The things to be burned being laid on the hot coals at A, the ſmoke muſt deſcend through thoſe coals, be converted into flame, which, after deſtroying the offenſive ſmell, came out at the end of the longer tube as mere heated air.

Plate II. Figure 1.

Whoever would repeat this experiment with ſucceſs, muſt take care that the part A, B, of the ſhort tube be quite full of burning coals, ſo that no part of the ſmoke may deſcend and paſs by them without going through them, and being converted into flame; and that the longer tube be ſo heated as that the current of aſcending hot air is eſtabliſhed in it before the things to be burnt are laid on the coals; otherwiſe there will be a diſappointment.

It does not appear either in the Memoirs of the Academy of Sciences, or Philoſophical Tranſactions of the Engliſh Royal Society, that any improvement was ever made of this ingenious experiment, by applying it to uſeful purpoſes. But there is a German book, entitled *Vulcanus Famulans*, by Joh. George Leutmann, P. D. printed at Wirtemberg in 1723, which deſcribes, among a great variety of other ſtoves for warming rooms, one which ſeems to have been formed on the ſame principle, and probably from the hint thereby given, though the French experiment is not mentioned. This book being ſcarce, I have tranſlated the chapter deſcribing the ſtove, viz.

"Vulcanus Famulans, by John George Leutmann, P. D.
"Wirtemberg, 1723.

"CHAP. VII.

"On a ſtove, which draws downwards.

"Here follows the deſcription of a ſort of ſtove, which "can eaſily be removed and again replaced at pleaſure. "This drives the fire down under itſelf, and gives no "ſmoke, but however a very unwholeſome vapour.

"In the figure, A is an iron veſſel like a fun-
Plate II. Figure 20. "nel, in diameter at the top about twelve inches, "at the bottom near the grate about five inches; "its height twelve inches. This is ſet on the barrel C, "which is ten inches diameter and two feet long, cloſed

"at

" at each end E E. From one end riſes a pipe or flue " about four inches diameter, on which other pieces of pipe " are ſet, which are gradually contracted to D, where the " opening is but about two inches. Thoſe pipes muſt to- " gether be at leaſt four feet high. B is an iron grate. " F F are iron handles guarded with wood, by which the " ſtove is to be lifted and moved. It ſtands on three legs. " Care muſt be taken to ſtop well all the joints, that no " ſmoke may leak through.

" When this ſtove is to be uſed, it muſt firſt be carried " into the kitchen and placed in the chimney near the fire. " There burning wood muſt be laid and left upon its grate " till the barrel C is warm, and the ſmoke no longer riſes " at A, but deſcends towards C. Then it is to be carried " into the room which it is to warm. When once the " barrel C is warm, freſh wood may be thrown into the " veſſel A as often as one pleaſes, the flame deſcends and " without ſmoke, which is ſo conſumed that only a va- " pour paſſes out at D.

" As this vapour is unwholeſome, and affects the head, " one may be freed from it, by fixing in the wall of the " room an inverted funnel, ſuch as people uſe to hang over " lamps, through which their ſmoke goes out as through " a chimney. This funnel carries out all the vapour cle- " verly, ſo that one finds no inconvenience from it, even " though the opening D be placed a ſpan below the mouth " of the ſaid funnel G. The neck of the funnel is better " when made gradually bending, than if turned in a right " angle.

" The cauſe of the draft downwards in the ſtove is the " preſſure of the outward air, which falling into the veſſel " A in a column of twelve inches diameter, finds only " a reſiſting paſſage at the grate B, of five inches, and " one at D, of two inches, which are much too weak " to drive it back again; beſides, A ſtands much higher " than B, and ſo the preſſure on it is greater and more

" forcible, and beats down the flame to that part where " it finds the leaſt reſiſtance. Carrying the machine firſt " to the kitchen fire for preparation, is on this account, " that in the beginning the fire and ſmoke naturally aſcend, " till the air in the cloſe barrel C is made thinner by the " warmth. When that veſſel is heated, the air in it is " rarefied, and then all the ſmoke and fire deſcends " under it.

" The wood ſhould be throughly dry, and cut into " pieces five or ſix inches long, to fit it for being thrown " into the funnel A." Thus far the German book.

It appears to me by Mr. Leutmann's explanation of the operation of this machine, that he did not underſtand the principles of it, whence I conclude he was not the inventor of it; and by the deſcription of it, wherein the opening at A is made ſo large, and the pipe E, D, ſo ſhort, I am perſuaded he never made nor ſaw the experiment, for the firſt ought to be much ſmaller and the laſt much higher, or it hardly will ſucceed. The carrying it in the kitchen, too, every time the fire ſhould happen to be out, muſt be ſo troubleſome, that it is not likely ever to have been in practice, and probably has never been ſhown but as a philoſophical experiment. The funnel for conveying the vapour out of the room, would beſides have been uncertain in its operation, as a wind blowing againſt its mouth would drive the vapour back.

The ſtove I am about to deſcribe, was alſo formed on the idea given by the French experiment, and completely carried into execution before I had any knowledge of the German invention; which I wonder ſhould remain ſo many years in a country where men are ſo ingenious in the management of fire, without receiving long ſince the improvements I have given it.

Description of the Parts.

A, the bottom plate which lies flat upon the hearth, with its partitions 1, 2, 3, 4, 5, 6, that are caſt with it, and a groove Z Z, in which are to ſlide, the bottom edges of the ſmall plates Y, Y, figure 12; which plates meeting at X cloſe the front. Plate II. Figure 2.

B 1, figure 3, is the cover plate ſhowing its under ſide, with the grooves 1, 2, 3, 4, 5, 6, to receive the top edges of the partitions that are fixed to the bottom plate. It ſhows alſo the grate W W, the bars of which are caſt in the plate, and a groove V V, which comes right over the groove Z Z, figure 2, receiving the upper edges of the ſmall ſliding plates Y Y, figure 12.

B 2, figure 4, ſhows the upper ſide of the ſame plate, with a ſquare impreſſion or groove for receiving the bottom mouldings T T T T of the three ſided box C, figure 5, which is caſt in one piece.

D, figure 6, its cover, ſhowing its under ſide with grooves to receive the upper edges S S S of the ſides of C, figure 5, alſo a groove R, R, which when the cover is put on comes right over another Q Q in C, figure 5, between which it is to ſlide.

E, figure 7, the front plate of the box.

P, a hole three inches diameter through the cover D, figure 6, over which hole ſtands the vaſe F, figure 8, which has a correſponding hole two inches diameter through its bottom.

The top of the vaſe opens at O, O, O, figure 8, and turns back upon a hinge behind when coals are to be put in; the vaſe has a grate within at N N of caſt iron H, figure 9, and a hole in the top one and a half inches diameter to admit air, and to receive the ornamental braſs guilt flame M, figure 10, which ſtands in that hole and, being itſelf hollow and open, ſuffers air to paſs through it to the fire.

G, figure 11, is a drawer of plate iron, that ſlips in between in the partitions 2 and 3, figure 2, to receive the falling

falling aſhes. It is concealed when the ſmall ſliding plates Y Y, figure 12, are ſhut together.

I, I, I, I, figure 8, is a niche built of brick in the chimney and plaſtered. It cloſes the chimney over the vaſe, but leaves two funnels one in each corner communicating with the bottom box K K, figure 2.

DIMENSIONS of the PARTS.

	Feet.	In.
Front of the bottom box, - -	2	0
Height of its partitions, - -	0	$4\frac{1}{4}$
Length of N° 1, 2, 3 and 4, each, -	1	3
Length of N° 5 and 6, each - -	0	$8\frac{1}{4}$
Breadth of the paſſage between N° 2 and 3,	0	6
Breadth of the other paſſages each, -	0	$3\frac{1}{2}$
Breadth of the grate, - -	0	$6\frac{1}{2}$
Length of ditto, - - -	0	8
Bottom moulding of box C, ſquare, -	1	0
Height of the ſides of ditto, - -	0	4
Length of the back ſide, - -	0	10
Length of the right and left ſides, each, -	0	$9\frac{1}{2}$
Length of the front plate E, where longeſt,	0	11
The cover D, ſquare, - -	0	12
Hole in ditto, diameter, - -	0	3
Sliding plates Y Y their length, each, -	1	0
——————their breadth, each, -	0	$4\frac{1}{4}$
Drawer G, its length, - - -	1	0
——————breadth, - - -	0	$5\frac{3}{4}$
——————depth, - - -	0	4
——————depth of its further end, only,	0	1
Grate H in the vaſe, its diameter to the extremity of its knobs, - - -	0	$5\frac{3}{4}$
Thickneſs of the bars at top, - -	0	$0\frac{1}{4}$
——————at bottom, leſs, -	0	0
Depth of the bars at the top, - -	0	$0\frac{3}{4}$
Height of the vaſe, - - -	1	6
Diameter of the opening O, O, in the clear,	0	8

Diameter

	Feet.	In.
Diameter of the air-hole at top, - -	0	1 1/2
——————of the flame hole at bottom, -	0	2

To fix this Machine.

Spread mortar on the hearth to bed the bottom plate A, then lay that plate, level, equally diſtant from each jamb, and projecting out as far as you think proper. Then puting ſome Windſor loam in the grooves of the cover B, lay that on: Trying the ſliding plates Y Y, to ſee if they move freely in the groves Z Z, V V, deſigned for them.

Then begin to build the niche, obſerving to leave the ſquare corners of the chimney unfilled; for they are to be funnels. And obſerve alſo to leave a free open communication between the paſſages at K K, and the bottom of thoſe funnels, and mind to cloſe the chimney above the top of the niche, that no air may paſs up that way. The concave back of the niche will reſt on the circular iron partition 1 A 4, figure 2, then with a little loam put on the box C over the grate, the open ſide of the box in front.

Then, with loam in three of its grooves, the groove R R being left clean, and brought directly over the groove Q Q in the box, put on the cover D, trying the front plate E, to ſee if it ſlides freely in thoſe grooves.

Laſtly, ſet on the vaſe, which has ſmall holes in the moulding of its bottom to receive two iron pins that riſe out of the plate D at I I, for the better keeping it ſteady.

Then putting in the grate H, which reſts on its three knobs H H H againſt the inſide of the vaſe, and ſlipping the drawer into its place; the machine is fit for uſe.

To uſe it.

Let the firſt fire be made after eight in the evening or before eight in the morning, for at thoſe times and between thoſe hours all night, there is uſually a draft up a chimney, though it has long been without fire; but between thoſe hours in the day there is often in a cold chim-

ney

ney a draft downwards, when if you attempt to kindle a fire, the ſmoke will come into the room.

But to be certain of your proper time, hold a flame over the air-hole at the top. If the flame is drawn ſtrongly down for a continuance, without whiffling, you may begin to kindle a fire.

Firſt put in a few charcoals on the grate H.

Lay ſome ſmall ſticks on the charcoals,

Lay ſome pieces of paper on the ſticks,

Kindle the paper with a candle,

Then ſhut down the top, and the air will paſs down through the air-hole, blow the flame of the paper down through the ſticks, kindle them, and their flame paſſing lower, kindles the charcoal.

When the charcoal is well kindled, lay on it the ſea-coals, obſerving not to choak the fire by putting on too much at firſt.

The flame deſcending through the hole in the bottom of the vaſe, and that in plate D into the box C paſſes down farther through the grate W W in plate B 1, then paſſes horizontally towards the back of the chimney; there dividing, and turning to the right and left, one part of it paſſes round the far end of the partition 2, then coming forward it turns round the near end of partition 1, then moving backward it arrives at the opening into the bottom of one of the upright corner funnels behind the niche, through which it aſcends into the chimney, thus heating that half of the box and that ſide of the niche. The other part of the divided flame paſſes round the far end of partition 3, round the near end of partition 4, and ſo into and up the other corner funnel, thus heating the other half of the box, and the other ſide of the niche. The vaſe itſelf, and the box C will alſo be very hot, and the air ſurrounding them being heated, and riſing, as it cannot get into the chimney, it ſpreads in the room, colder air ſucceeding

ſucceeding is warmed in its turn, riſes and ſpreads, till by the continual circulation the whole is warmed.

If you ſhould have occaſion to make your firſt fire at hours not ſo convenient as thoſe above mentioned, and when the chimney does not draw, do not begin it in the vaſe, but in one or more of the paſſages of the lower plate, firſt covering the mouth of the vaſe. After the chimney has drawn a while with the fire thus low, and begins to be a little warm, you may cloſe thoſe paſſages and kindle another fire in the box C, leaving its ſliding ſhutter a little open; and when you find after ſome time that the chimney being warmed draws forcibly, you may ſhut that paſſage, open your vaſe, and kindle your fire there, as above directed. The chimney well warmed by the firſt day's fire will continue to draw conſtantly all winter, if fires are made daily.

You will, in the management of your fire, have need of the following implements:

A pair of ſmall light tongs, twelve or fifteen inches long, plate II, figure 13.

A light poker about the ſame length with a flat broad point, figure 14.

A rake to draw aſhes out of the paſſages of the lower plate, where the lighter kind eſcaping the aſh-box will gather by degrees, and perhaps once in a week or ten days require being removed, figure 15.

And a fork with its prongs wide enough to ſlip on the neck of the vaſe cover, in order to raiſe and open it when hot, to put in freſh coals, figure 16.

In the management of this ſtove there are certain precautions to be obſerved, at firſt with attention, till they become habitual. To avoid the inconvenience of ſmoke, ſee that the grate H be clear before you begin to light a freſh fire. If you find it clogged with cinders and aſhes, turn it up with your tongs and let them fall upon the grate below; the aſhes will go through it, and the cinders may

be raked off and returned into the vaſe when you would burn them. Then ſee that all the ſliding plates are in their places and cloſe ſhut, that no air may enter the ſtove but through the round opening at the top of the vaſe. And to avoid the inconvenience of duſt from the aſhes, let the aſh-drawer be taken out of the room to be emptied; and when you rake the paſſages, do it when the draft of the air is ſtrong inwards, and put the aſhes carefully into the aſh-box, that remaining in its place.

If being about to go abroad, you would prevent your fire burning in your abſence, you may do it by taking the braſs flame from the top of the vaſe, and covering the paſſage with a round tin plate, which will prevent the entry of more air than barely ſufficient to keep a few of the coals alive. When you return, though ſome hours abſent, by taking off the tin plate and admitting the air, your fire will ſoon be recovered.

The effect of this machine, well managed, is to burn not only the coals, but all the ſmoke of the coals, ſo that while the fire is burning, if you go out and obſerve the top of your chimney, you will ſee no ſmoke iſſuing, nor any thing but clear warm air, which as uſual makes the bodies ſeen through it appear waving.

But let none imagine from this, that it may be a cure for bad or ſmoky chimneys, much leſs, that as it burns the ſmoke it may be uſed in a room that has no chimney. 'Tis by the help of a good chimney, the higher the better, that it produces its effect; and though a flue of plate iron ſufficiently high might be raiſed in a very lofty room, the management to prevent all diſagreeable vapour would be too nice for common practice, and ſmall errors would have unpleaſing conſequences.

It is certain that clean iron yields no offenſive ſmell when heated. Whatever of that kind you perceive, where there are iron ſtoves, proceeds therefore from ſome foulneſs burning or fuming on their ſurface. They ſhould

therefore never be ſpit upon, or greaſed, nor ſhould any duſt be ſuffered to lie upon them. But as the greateſt care will not always prevent theſe things, it is well once a week to waſh the ſtove with ſoap lees and a bruſh, rinſing it with clean water.

The Advantages of this Stove.

1. The chimney does not grow foul, nor ever need ſweeping; for as no ſmoke enters it, no ſoot can form in it.

2. The air heated over common fires inſtantly quits the room and goes up the chimney with the ſmoke; but in the ſtove, it is obliged to deſcend in flame and paſs through the long winding horizontal paſſages, communicating its heat to a body of iron plate, which having thus time to receive the heat, communicates the ſame to the air of the room, and thereby warms it to a greater degree.

3. The whole of the fuel is conſume d by beingturned into flame, and you have the benefit of its heat, whereas in common chimneys a great part goes away in ſmoke which you ſee as it riſes, but it affords you no rays of warmth. One may obtain ſome notion of the quantity of fuel thus waſted in ſmoke, by reflecting on the quantity of ſoot that a few weeks firing will lodge againſt the ſides of the chimney, and yet this is formed only of thoſe particles of the column of ſmoke that happen to touch the ſides in its aſcent. How much more muſt have paſſed off in the air? And we know that this ſoot is ſtill fuel; for it will burn and flame as ſuch, and when hard caked together is indeed very like and almoſt as ſolid as the coal it proceeds from. The deſtruction of your fuel goes on nearly in the ſame quantity whether in ſmoke or in flame: but there is no compariſon in the difference of heat given. Obſerve when freſh coals are firſt put on your fire, what a body of ſmoke ariſes. This ſmoke is for a long time too cold to take flame. If you then plunge a burning candle into it, the candle inſtead of inflaming the ſmoke will in-

ſtantly be itſelf extinguiſhed. Smoke muſt have a certain degree of heat to be inflammable. As ſoon as it has acquired that degree, the approach of a candle will inflame the whole body, and you will be very ſenſible of the difference of the heat it gives. A ſtill eaſier experiment may be made with the candle itſelf. Hold your hand near the ſide of its flame, and obſerve the heat it gives; then blow it out, the hand remaining in the ſame place, and obſerve what heat may be given by the ſmoke that riſes from the ſtill burning ſnuff. You will find it very little. And yet that ſmoke has in it the ſubſtance of ſo much flame, and will inſtantly produce it, if you hold another candle above it ſo as to kindle it. Now the ſmoke from the freſh coals laid on this ſtove, inſtead of aſcending and leaving the fire while too cold to burn, being obliged to deſcend through the burning coals, receives among them that degree of heat which converts it into flame, and the heat of that flame is communicated to the air of the room, as above explained.

4. The flame from the freſh coals laid on in this ſtove, deſcending through the coals already ignited, preſerves them long from conſuming, and continues them in the ſtate of red coals as long as the flame continues that ſurrounds them, by which means the fires made in this ſtove are of much longer duration than in any other, and fewer coals are therefore neceſſary for a day. This is a very material advantage indeed. That flame ſhould be a kind of pickle, to preſerve burning coals from conſuming, may ſeem a paradox to many, and very unlikely to be true, as it appeared to me the firſt time I obſerved the fact. I muſt therefore relate the circumſtances, and ſhall mention an eaſy experiment, by which my reader may be in poſſeſſion of every thing neceſſary to the underſtanding of it. In the firſt trial I made of this kind of ſtove, which was conſtructed of thin plate iron, I had inſtead of the vaſe a kind of inverted pyramid like a mill-hopper; and fearing at

firſt

firſt that the ſmall grate contained in it might be clogged by cynders, and the paſſage of the flame ſometimes obſtructed, I ordered a little door near the grate, by means of which I might on occaſion clear it. Though after the ſtove was made, and before I tried it, I began to think this precaution ſuperfluous, from an imagination, that the flame being contracted in the narrow part where the grate was placed, would be more powerful in conſuming what it ſhould there meet with, and that any cynders between or near the bars would be preſently deſtroyed and the paſſage opened. After the ſtove was fixed and in action, I had a pleaſure now and then in opening that door a little, to ſee through the crevice how the flame deſcended among the red coals, and obſerving once a ſingle coal lodged on the bars in the middle of the focus, a fancy took me to obſerve by my watch in how ſhort a time it would be conſumed. I looked at it long without perceiving it to be at all diminiſhed, which ſurpriſed me greatly. At length it occurred to me, that I and many others had ſeen the ſame thing thouſands of times, in the conſervation of the red coal formed in the ſnuff of a burning candle, which while envelloped in flame, and thereby prevented from the contact of paſſing air, is long continued and augments inſtead of diminiſhing, ſo that we are often obliged to remove it by the ſnuffers, or bend it out of the flame into the air, where it conſumes preſently to aſhes. I then ſuppoſed that to conſume a body by fire, paſſing air was neceſſary to receive and carry off the ſeparated particles of the body; and that the air paſſing in the flame of my ſtove, and in the flame of a candle, being already ſaturated with ſuch particles, could not receive more, and therefore left the coal undiminiſhed as long as the outward air was prevented from coming to it by the ſurrounding flame, which kept it in a ſituation ſomewhat like that of charcoal in a well luted crucible, which, though long kept in a ſtrong fire, comes out unconſumed.

An

An eaſy experiment will ſatisfy any one of this conſerving power of flame envelloping red coal. Take a ſmall ſtick of deal or other wood the ſize of a gooſe quill, and hold it horizontally and ſteadily in the flame of the candle above the wick, without touching it, but in the body of the flame. The wood will firſt be inflamed, and burn beyond the edge of the flame of the candle, perhaps a quarter of an inch. When the flame of the wood goes out, it will leave a red coal at the end of the ſtick, part of which will be in the flame of the candle and part out in the air. In a minute or two you will perceive the coal in the air diminiſh gradually, ſo as to form a neck; while the part in the flame continues of its firſt ſize, and at length the neck being quite conſumed it drops off; and by rolling it between your fingers when extinguiſhed you will find it ſtill a ſolid coal.

However, as one cannot be always putting on freſh fuel in this ſtove to furniſh a continual flame as is done in a candle, the air in the intervals of time gets at the red coals and conſumes them. Yet the conſervation while it laſted, ſo much delayed the conſumption of the coals, that two fires, one made in the morning, and the other in the afternoon, each made by only a hatfull of coals, were ſufficient to keep my writing room, about ſixteen feet ſquare and ten high, warm a whole day. The fire kindled at ſeven in the morning would burn till noon; and all the iron of the machine with the walls of the niche being thereby heated, the room kept warm till evening, when another ſmaller fire kindled kept it warm till midnight.

Inſtead of the ſliding plate E, which ſhuts the front of the box C, I ſometimes uſed another which had a pane of glaſs, or, which is better, of Muſcovy talc, that the flame might be ſeen deſcending from the bottom of the vaſe and paſſing in a column through the box C, into the cavities of the bottom plate, like water falling from a funnel, admirable to ſuch as are not acquainted with the nature of the machine, and in itſelf a pleaſing ſpectacle. Every

Every utenſil, however properly contrived to ſerve its purpoſe, requires ſome practice before it can be uſed adroitly. Put into the hands of a man for the firſt time, a gimblet or a hammer, (very ſimple inſtruments) and tell him the uſe of them, he ſhall neither bore a hole or drive a nail with the dexterity or ſucceſs of another who has been a little accuſtomed to handle them. The beginner therefore in the uſe of this machine, will do well not to be diſcouraged with little accidents that may ariſe at firſt from his want of experience. Being ſomewhat complex, it requires as already ſaid a variety of attentions; habit will render them unneceſſary. And the ſtudious man who is much in his chamber, and has a pleaſure in managing his own fire, will ſoon find this a machine moſt comfortable and delightful. To others who leave their fires to the care of ignorant ſervants, I do not recommend it. They will with difficulty acquire the knowledge neceſſary, and will make frequent blunders that will fill your room with ſmoke. It is therefore by no means fit for common uſe in families. It may be adviſeable to begin with the flaming kind of ſtone coal, which is large, and, not caking together, is not ſo apt to clog the grate. After ſome experience, any kind of coal may be uſed, and with this advantage, that no ſmell, even from the moſt ſulphurous kind can come into your room, the current of air being conſtantly into the vaſe, where too that ſmell is all conſumed.

The vaſe form was choſen as being elegant in itſelf, and very proper for burning of coals: Where wood is the uſual fuel, and muſt be burnt in pieces of ſome length, a long ſquare cheſt may be ſubſtituted, in which A is the cover opening by a hinge behind, B the grate, C the hearth box with its diviſions as in the other, D the plan of the cheſt, E the long narrow grate. This I have not tried, but the vaſe machine was compleated in 1771, and uſed by me in London three winters, and one afterwards in America, much to my ſatisfaction; and I have not yet thought

Plate 2. Figure 17.

thought of any improvement it may be capable of, though ſuch may occur to others. For common uſe, while in France, I have contrived another grate for coals, which has in part the ſame property of burning the ſmoke and preſerving the red coals longer by the flame, though not ſo completely, as in the vaſe, yet ſufficiently to be very uſeful, which I ſhall now deſcribe as follows.

A, is a round grate, one foot (French) in diameter, and eight inches deep between the bars and the back; the ſides and back of plate iron; the ſides having holes of half an inch diameter diſtant 3 or 4 inches from each other, to let in air for enlivening the fire. The back without holes. The ſides do not meet at top nor at bottom by eight inches: that ſquare is filled by grates of ſmall bars croſſing front to back to let in air below, and let out the ſmoke or flame above. The three middle bars of the front grate are fixed, the upper and lower may be taken out and put in at pleaſure, when hot, with a pair of pincers. This round grate turns upon an axis, ſupported by the crotchet B, the ſtem of which is an inverted conical tube five inches deep, which comes on as many inches upon a pin that fits it, and which is fixed upright in a caſt iron plate D, that lies upon the hearth; in the middle of the top and bottom grates are fixed ſmall upright pieces E E about an inch high, which as the whole is turned on its axis ſtop it when the grate is perpendicular. Figure 19 is another view of the ſame machine. Plate 2. Figure 18.

In making the firſt fire in a morning with this grate, there is nothing particular to be obſerved. It is made as in other grates, the coals being put in above, after taking out the upper bar, and replacing it when they are in. The round figure of the fire when thoroughly kindled is agreeable, it repreſents the great giver of warmth to our ſyſtem. As it burns down and leaves a vacancy above, which you would fill with freſh coals, the upper bar is to be taken out, and afterwards replaced. The freſh coals while the

grate

grate continues in the ſame poſition, will throw up as uſual a body of thick ſmoke. But every one accuſtomed to coal fires in common grates, muſt have obſerved that pieces of freſh coal ſtuck in below among the red coals have their ſmoke ſo heated as that it becomes flame as faſt as it is produced, which flame riſes among the coals and enlivens the appearance of the fire. Here then is the uſe of this ſwivel grate. By a puſh with your tongs or poker, you turn it on its pin till it faces the back of the chimney, then turn it over on its axis gently till it again faces the room, whereby all the freſh coals will be found under the live coals, and the greater part of the ſmoke ariſing from the freſh coals will in its paſſage through the live ones be heated ſo as to be converted into flame: Whence you have much more heat from them, and your red coals are longer preſerved from conſuming. I conceive this conſtruction, though not ſo complete a conſumer of all the ſmoke as the vaſe, yet to be fitter for common uſe, and very advantageous. It gives too a full ſight of the fire, always a pleaſing object, which we have not in the other. It may with a touch be turned more or leſs from any one of the company that deſires to have leſs of its heat, or preſented full to one juſt come out of the cold. And ſupported in a horizontal poſition, a tea-kettle may be boiled on it.

The author's deſcription of his Pennſylvania fire-place, firſt publiſhed in 1744, having fallen into the hands of workmen in Europe, who did not, it ſeems, well comprehend the principles of that machine, it was much diſfigured in their imitations of it; and one of its main intentions, that of admitting a ſufficient quantity of freſh air warmed in entering through the air-box, nearly defeated, by a pretended improvement, in leſſening its paſſages to make more room for coals in a grate. On pretence of ſuch improvements, they obtained patents for the invention, and for a while made great profit by the ſale, till the public became ſenſible of that defect, in the ex-

pected operation. If the same thing should be attempted with this vase stove, it will be well for the buyer to examine thoroughly such pretended improvements, lest, being the mere productions of ignorance, they diminish or defeat the advantages of the machine, and produce inconvenience and disappointment.

The method of burning smoke, by obliging it to descend through hot coals, may be of great use in heating the walls of a hot-house. In the common way, the horizontal passages or flues that are made to go and return in those walls, lose a great deal of their effect when they come to be foul with soot; for a thick blanket-like lining of soot prevents much of the hot air from touching and heating the brick work in its passage, so that more fire must be made as the flue grows fouler: But by burning the smoke they are kept always clean. The same method may also be of great advantage to those businesses in which large coppers or caldrons are to be heated.

Written at Sea, 1785.

N° VII.

A Theory of Lightening and Thunder Storms, by ANDREW OLIVER, *Esq. of Salem in the State of Massachusetts.*

Read January, 1774. IT has been generally, and, considering the phenomena themselves, very naturally supposed, that the electric charges which are exhibited in repeated flashes of lightening during a thunder storm, are previously accumulated in the vapors which constitute the cloud; and that these vapors, when by any means they become either over-charged with electric matter, or are deprived of their natural

natural quantities of it*, diſcharge their ſurpluſage to, or receive the neceſſary ſupplies from, either the earth or the neighbouring clouds, in ſucceſſive exploſions, till an equilibrium is reſtored between them. But I ſhall endeavour in the following pages to prove, that theſe charges reſide, not in the cloud or vapors of which it conſiſts, but in the air which ſuſtains them; and that, previous to the formation of the cloud, or even the aſcent of the vapors of which it is formed. But, in order to convey my ideas upon this ſubject with perſpicuity, I find it neceſſary to introduce them with a quotation from doctor *Franklin*'s letters on electricity, in which the doctor compares water, whether in its natural ſtate, or rarefied into vapors, to a ſponge; and the electric fluid, in connection with it, to water applied to the ſponge.

" When a ſponge (ſays he) is ſomewhat condenſed by " being ſqueezed between the fingers, it will not receive " and retain ſo much water as when it is in its more looſe " and open ſtate. If more ſqueezed and condenſed, ſome " of the water will come out of its inner parts, and flow " on the ſurface. If the preſſure of the fingers be intire- " ly removed, the ſponge will not only reſume what was " lately forced out, but attract an additional quantity. As " the ſponge in its rarer ſtate will *naturally* attract and " abſorb *more* water; and in its denſer ſtate will *naturally* " attract and abſorb *leſs* water; we may call the quantity " it abſorbs in either ſtate, its *natural quantity*, the ſtate " being conſidered."

The doctor then ſuppoſes, " that what the ſponge is to " water, the ſame is water to the electric fluid;—that " when a portion of water is in its common denſe ſtate, " it can hold no more electric fluid than it has; if any be " added it ſpreads upon the ſurface." He adds, " when " the ſame portion of water is rarefied into vapor and forms

 " a cloud,

* A body is ſaid to be electrically charged, whenever it has either *more* or *leſs* than its natural quantity of electric matter.

" a cloud, it is then capable of receiving and abſorbing a " much greater quantity, as there is room for each parti- " cle to have an electric atmoſphere. Thus water in its " rarefied ſtate, or in the form of a cloud, will be in a " negative ſtate of electricity; it will have leſs than its " *natural quantity*, that is, leſs than it is naturally capable " of attracting and abſorbing in that ſtate*."

The foregoing paſſages I have copied *verbatim* from that celebrated electrician, as I purpoſe in the courſe of this eſſay to avail myſelf of his idea of the ſponge, in order to illuſtrate a different theory of thunder clouds, which I now beg leave, though with diffidence of my own judgment, and with all due deference to that of ſo great a man, to ſubſtitute in the room of the foregoing; which I muſt confeſs at firſt ſight carries great appearance of probability with it, and is highly corroborated by the curious and beautiful experiment the doctor made with the ſilver cann, braſs chain, and lock of cotton†.

But in reading doctor *Prieſtley*'s hiſtory of electricity, ſome thoughts of ſignior *Beccaria* occurred, which ſatisfied me that this hypotheſis, however ingenious and plauſible, was inſufficient for the purpoſe of accounting for the riſe and phenomena of thunder ſtorms, the frequent extent and violence of which ſeem to require a more general cauſe than that hinted above, to ſupply them with ſufficient quantities of electric matter.

" Conſidering the vaſt quantity of electric fire that ap- " pears in the moſt ſimple thunder ſtorms (ſays doctor " Prieſtly‡) ſignior *Beccaria* thinks it impoſſible that any " cloud, or number of clouds, ſhould ever contain it all, " ſo as either to diſcharge or receive it. Beſides, during " the progreſs and increaſe of the ſtorm, though the light- " ening frequently ſtruck to the earth, the ſame clouds " were

* Franklin's Letters, page 119.
† Page 121.
‡ Prieſtley's Hiſtory of Electricity, page 325.

" were the next moment ready to make a ſtill greater diſ-
" charge, and his apparatus continued to be as much af-
" fected as ever. The clouds muſt conſequently have *re-*
" *ceived* at one place the moment that a *diſcharge* was
" made from them in another."

Signior *Beccaria* accounts for this vaſt exhibition of electric fire from a thunder cloud, by ſuppoſing that ſome parts of the earth may become more highly charged with the electric fluid than others, and that great quantities of it do ſometimes ruſh out of particular parts, and riſe through the air into the higher regions of the atmoſphere; other parts of the earth becoming caſually deſtitute of their natural quantity of the fluid at the ſame time, and ready to receive it: That a chain of clouds nearly contiguous, or a ſingle cloud extending from one of theſe regions to another, in an oppoſite ſtate, might ſerve as a conductor or conductors to reſtore the electric equilibrium between them, which would equally cauſe thunder and lightening in both regions, and throughout the intermediate clouds*. Here doctor *Prieſtley* juſtly obſerves, that " the greateſt difficul-
" ty attending this theory of the origin of thunder ſtorms
" relates to the *collection* and *inſulation* of electric matter
" within the body of the earth." With regard to the *collection*, the doctor obſerves that his author " has nothing
" particularly to ſay:" Nor indeed without a previous *inſulation* of thoſe parts of the earth which may be concerned in the production of the phenomena, can any ſuch *collection* take place. Now if we conſider that in order to have two regions of the earth thus inſulated, and of ſufficient dimenſions, one to ſupply, and the other to receive the quantities of electric fire diſcharged during one thunder ſtorm of any extent and continuance, the parts inſulated muſt be not ſuperficial regions, but muſt reach to a conſiderable depth; and we muſt ſuppoſe, with doctor Prieſtley, " that the electric matter which forms and ani-
" mates the thunder cloud, iſſues from places far below
" the

* Ibid.

" the ſurface of the earth, and that it buries itſelf there*." But, with deference to the judgment of that unwearied friend to ſcience, I apprehend that ſuch an inſulation is hardly conſiſtent with that diſtribution of conductors, eſpecially of water, which provident nature has made through all parts of our globe; the higheſt mountains being furniſhed with *internal* ſprings and fountains, and watered *externally* by rivulets, which derive their origin from condenſing miſts or melting ſnows upon their ſummits: While the ſurface of the earth in general, not excepting the moſt ſandy deſerts, affords ſupplies of water to thoſe who will be at the pains of digging for it. If then the vapors which conſtitute the cloud are, of themſelves, incapable of furniſhing ſuch quantities of electric matter as are neceſſary for the repeated diſcharges in a ſevere thunder ſtorm, as ſignior *Beccaria* thinks they are, and as ſeems to me indubitable; and if the inſulations of large portions of the ſurface or exterior parts of the earth, which are abſolutely neceſſary to ſupport *Beccaria*'s hypotheſis, cannot take place; which, how they can in our terraqueous maſs, is difficult to conceive, conſiſtently with the *hitherto* diſcovered properties of the electric fluid: We muſt ſeek for ſome other ſubſtance in nature which may be capable of affording thoſe reiterated ſupplies, of that powerful element which are uſually exhibited in a thunder ſtorm. This I preſume, we ſhall find in the atmoſphere over our heads; not in the *vapors* which float therein, but in the *air* itſelf which ſuſtains them.

Air is by electricians juſtly claſſed with *electric* ſubſtances, as it poſſeſſes the ſame general properties in common with others of that denomination, particular inſtances of which may occur in the following pages; wherein I ſhall endeavour to prove,

I. That the *electric capacity* of air is leſſened by condenſation.

II. That *this* capacity is increaſed by heat.

Premiſing

* Prieſtley, page 335.

Premiſing that by *air* I here intend *that* fluid in its common compreſſed ſtate with us near the ſurface of the earth; and by its *electric capacity*, that ſtate of it which diſpoſes it, under any circumſtances whatever, "to attract, abſorb and retain," what doctor *Franklin* calls its *natural quantity*, or the quantity which is *natural* to it in that ſtate.

I. I ſhall endeavour to prove that the *electric capacity* of air is leſſened by condenſation.

That a change of denſity in air produces alſo a change in its electric capacity (as above defined), follows from ſome experiments of monſieur de *Faye* and doctor *Prieſtley*, the former of whom found, upon repeated trials, that no electricity could be excited by the friction of a glaſs tube in which the air was condenſed*. The doctor, repeating the experiments with ſome variation, found, that when one additional atmoſphere was forced into the tube, the electricity excited by rubbing it was ſcarcely diſcernable. Now, though the effect was a ſuſpenſion of the operation of the excited tube *without*, the cauſe was evidently the condenſed ſtate of the air *within;* which may be accounted for if we conſider, that although it is certain from many experiments that glaſs is abſolutely impermeable to the electric fluid, inſomuch that it cannot force its way through a pane of glaſs, or the ſides of a phial, without breaking the glaſs, as was the caſe in thoſe ſpontaneous diſcharges of ſeveral of the jars in the electrical battery mentioned by doctor *Prieſtley*†; yet it is as certain, that this impermeability of the glaſs to the fluid itſelf, is no obſtruction to the operation of that repellent power upon which the viſible effects of this element ſeem principally to depend; which power undeniably acts from one ſide of the glaſs, through the very ſubſtance of it, upon the ſame fluid on the other ſide, provided there be any other ſubſtance on that ſide capable of receiving it when thus repelled.

This is the caſe in the *Leyden* experiment in every form in which it can be made; the charge given to one ſide of the

* Page 50. † Page 489.

the glaſs, repelling and throwing off an equal quantity of the electric fluid from the oppoſite ſurface, through the non-electric coating in contact with it; nor can any charge be given to either ſide without a proportional diſcharge from the other. In like manner, when an uncoated tube is excited by friction, a quantity of the fluid, equal to that which is excited and condenſed upon the outer ſurface, is thrown out from the inner, provided there is any ſubſtance within in a capacity to receive and abſorb it, without which no excitation can take place. "A glaſs tube, "out of which the air is exhauſted, diſcovers no ſigns of "electricity outwards*," there being no ſubſtance within capable of receiving and abſorbing the fluid from the inner ſurface, which though repelled from it inwards during the operation, yet returns to it again inſtantly upon a ceſſation of the action of the rubber without. But upon a readmiſſion of air the excitation is eaſy, and is attended with the uſual effects. Air then, which is the only ſubſtance admitted (excepting perhaps a few ſtraggling vapors which float in it) receives and abſorbs a ſufficient quantity of the electric fluid from the inner ſurface to permit an excitation of the tube which contains it. But as we have ſeen that air, when condenſed within, prevents the viſible effects of an excitation, equally with a total vacuity, we may adopt the idea of doctor *Franklin*, *mutatis mutandis*, and conlcude that "what the ſponge is to water "the ſame is *air* to the electric fluid:" At leaſt that this capacity of air if leſſened by condenſation in a manner, not indeed perfectly ſimilar, but, ſomewhat analogous to that in which the capacity of a ſponge to receive and retain water is leſſened by compreſſion. Agreeably to which idea, the condenſed air within the tube, having its electric capacity filled and even crowded with the electric matter, will receive none from the inner ſurface, which, on the contrary, is thereby prevented from being forced out of it, without which

* Prieſtley's hiſtory of electricity, page 550.

which none can be forced into or condenſed upon the outer ſurface, ſo as to exhibit any ſigns of electricity; as obſerved before,

II. I ſhall endeavour to prove that the electric capacity of air is increaſed by heat.

This alſo appears probable, at leaſt, from the above cited experiments of doctor *Prieſtley*; for after the air in his tube had had this capacity ſo far diminiſhed by condenſation as not to permit an excitation without, that capacity, together with the conſequent excitability of the tube, was reſtored by the action of heat upon the included air. "Repeating my attempts (ſays he) to excite the tube above "mentioned, I found that, after very hard rubbing, it began to act a little, and that its virtue increaſed with the "labour. Thinking it might be the warmth which produced this effect, I held the tube to the fire and found "that when it was pretty hot, it would act almoſt as well "as when it contained no more than its uſual quantity "of air*."

In page 553, doctor *Prieſtley* tells us that ſome of his electrical friends were of opinion, "that the reaſon why "a tube with condenſed air in it cannot be excited is, that "the denſe air within prevents the electric fluid from being forced out of the inſide of the tube, without which "none can be forced into the outſide; and that heating "the tube makes the air within leſs electrical." That is, as I conceive their meaning, puts it in a capacity to receive and abſorb more of the electric fluid than it could otherwiſe do in that condenſed ſtate. The doctor indeed queries by way of objection to the foregoing ſolution,—"How upon this principle can a ſolid ſtick of glaſs be excited?" To which I would anſwer, that poſſibly, when a ſolid ſtick of glaſs is excited, as much of the electric fluid may be drawn out of one ſide of it as is thrown into, or condenſed upon the other; if ſo, although it may ſhew equal ſigns of electricity on both ſides, yet one ſide will be in a

* Page 551.

poſitive, the other in a negative ſtate; when it will exactly reſemble the curious ſtone called the *tourmalin*, by ſome *lapis electricus*, which doctor *Prieſtley* ſays * "has "always, *at the ſame time*, a poſitive and a negative elec- "tricity; one of its ſides being in one ſtate, and the other "in the oppoſite;" which does not depend upon the external form "of the ſtone." But the truth of this ſolution muſt be determined by future experiments.

That the electrical ſtate of the air is liable to be affected by heat, is further evident from a courſe of experiments which were made by the abbé *Mazeas*, with an apparatus that was conſtructed ſolely with a view of determining the electricity of the atmoſphere, anno 1753†. With this apparatus the abbé obſerved, that from the 17th of June, when he began his experiments, the electricity of the air was ſenſibly felt every day, *from ſun riſe till ſeven or eight o'clock in the evening*, when the weather was *dry*; but that in the drieſt *nights* of that ſummer he could diſcover no ſigns of electricity in the air, nor till the morning, when the ſun began to appear above the horizon, and that "they vaniſhed again in the evening, about half an "hour after ſun ſet;" and further, "that the *ſtrongeſt* "common electricity of the atmoſphere, during the ſum- "mer, was perceived in the month of *July* on a *very dry* "day, the heavens being very clear, and the ſun *extreme- "ly hot*."

Now, as this electricity of the air was ſenſible only during *day light*, no electricity being diſcoverable therein even in the *drieſt nights*, and as the air exhibited the *ſtrongeſt* ſigns of electricity when the ſun ſhone *extremely hot*; is not the concluſion unavoidable, that heat ſomehow affects the electric capacity of air, either enlarging it, and thereby diſpoſing the air to attract, receive and abſorb greater quantities of electric matter than it is capable of abſorbing in its natural ſtate; or ſuperadding to its *natural quantity* more than it can abſorb, and thereby diſpoſing it to throw off

* Page 299. † Page 342.

off the redundancy upon any objects which may be in a ſituation to receive it? One or the other ſeems neceſſarily to follow, but the former is moſt agreeable to doctor *Prieſtley*'s experiment of the condenſed air in the tube above mentioned, and is perfectly conſonant with the obſervations of doctor *Franklin*, Mr. *Kinnerſley* and others, that thunder clouds are generally in the *negative* ſtate of electricity*. But more upon this head hereafter. I would however obſerve here, that many, and perhaps all other electric ſubſtances, even the moſt firm and ſolid, as well as air, are liable to have their electric capacities thus diverſified by heat, more particularly the tourmalin above mentioned. But as, in treating of the properties of this ſtone, doctor *Prieſtley* has thought it deſerving of a diſtinct ſection in his electric hiſtory, to that I ſhall refer the reader for a particular account of them †; wherein he will find a diſcovery made by Meſſrs *Canton* and *Wilſon*, that theſe properties are not peculiar to the tourmalin, but that many gems have a natural diſpoſition to afford the ſame appearances; from whence we may conclude as above, by analogy, that all electric ſubſtances are, more or leſs, affected in like manner, by the ſame cauſe. But to return to the ſubject.

If from the foregoing conſiderations the reader ſhould be ſatisfied, that the *electric capacity* of air, in its condenſed ſtate in the lower regions of the atmoſphere, is liable to be diminiſhed by a further condenſation, and that, *cæteris paribus*, it is increaſed by heat *et vice verſa*; the ſolution of the phenomena of thunder and lightening, to his ſatisfaction, upon electrical principles, will perhaps be no difficult taſk.

For let us conceive a region of the atmoſphere, extending over a large tract of country, to be rarefied and heat-

* Epitome of Phil. Tranſ. Gent. Mag. Sept. 1773, page 447. Mr. Henley thinks cold electrifies the atmoſphere poſitively, and thence conjectures that heat electrifies it negatively. His concluſions are founded upon a courſe of experiments.

† Page 297.

ed during a hot ſummer's day, not only by the paſſage of the ſun's direct rays through it, and by the reflectinoof thoſe rays from the ſurface of the earth into it; but chiefly, by the communication of the heat acquired by that ſurface to it: The *electric capacity* of that region of air would be increaſed, both on account of the heat it undergoes, and of the rarefaction conſequent upon that heat: It will then have leſs than its *natural quantity*, or the quantity it is *naturally* diſpoſed to receive and abſorb in that ſtate; it will conſequently be, in the language of electricians, *negatively* electriſed, or in a craving ſtate, requiring and forcing ſupplies from all ſubſtances capable of affording them, provided it be itſelf in a condition to receive them. But, however craving, it cannot receive thoſe ſupplies from the neighbouring regions of the atmoſphere, while thoſe regions ſeverally remain in the ſtate of pure air, even ſuppoſing the latter to poſſeſs more than their *natural quantities*, and thereby as much diſpoſed to impart, as the former is to receive them, without the intervention of non-electric conductors; and that, owing to the impermeability of air, as ſuch, to the electric fluid. This I ſhall endeavour, 1. To illuſtrate by experiments made with glaſs. 2. To prove by experiments made upon air itſelf.

1. If a pane of glaſs be coated on both ſides, by the application of plates of tin to them, the glaſs may be charged in the ſame manner as the *Leyden* phial; when, after the removal of the plates, no diſcharge having previouſly taken place, both ſides of the glaſs will remain charged, one poſitively, the other negatively; the former having more than its *natural quantity*, the latter being proportionably deficient, and in a craving ſtate. Theſe ſtates both ſurfaces will obſtinately maintain for a long time: Nor do I know of any method of reſtoring the electric equilibrium between them, but, either to immerſe the pane in water or ſome other non-electric fluid, which will do it inſtantly, and ſilently; or to reapply the metalline coatings to both ſides

ſides as they were placed at firſt, with a good conductor introduced between them, which will anſwer the ſame purpoſe, and be attended with an exploſion, or ſmart ſpark and ſnap; or laſtly, to place it in a ſituation where it may be expoſed to air replete with moiſt vapors, where, after ſome time, the vapors will, by condenſing upon each ſide, furniſh it with a moiſture equivalent to a non-electric coating, while the vapors which remain in the ſurrounding air will, by continually impinging upon and receding from the two ſurfaces, at length reſtore both to their natural ſtate.

It is evident from the foregoing experiment, *Firſt*, That the charges reſide in the glaſs itſelf, as they remain after the coatings are removed. *Secondly*, That the oppoſite ſides have a very ſtrong propenſity, one to give, the other to receive the fluid, and thereby to reſtore the electric equilibrium between themſelves; which is done with violence, as obſerved above, when they are put in a condition of doing it by the reapplication of the metalline coatings, with a conductor between them, and *Laſtly*, That notwithſtanding the violent propenſity in the ſides of the glaſs, to reſtore themſelves and each other to their natural electric ſtates, and the ſmall diſtance between them, they can never effect it, without the intervention of non-electric conductors.

2. I ſhall now ſhew by other experiments, that different regions or ſtrata of air *may* become charged, both poſitively and negatively, in the ſame manner as the ſides of the pane of glaſs were in the foregoing; and that the effects of ſuch charges are preciſely the ſame.

Meſſrs *Wilkie* and *Æpinus* at *Berlin*, having the hint naturally ſuggeſted to them by a previous courſe of experiments, endeavoured to give the electrical ſhock by means of *air*, in the ſame manner in which it may be given by *glaſs*; "in which after making ſeveral attempts (ſays doc-
"tor Prieſtley*) they at length ſucceeded, by ſuſpending
"large

* Page 242.

" large boards of wood covered with tin, with the flat ſides " towards one another, and at ſome inches aſunder. For " they found, that upon electrifying one of the boards " poſitively, the other was always negative. But the diſ- " covery was made complete and indiſputable by a perſon's " touching one of the plates with one hand, and bringing " his other hand to the other plate; for he then received " a ſhock through his body exactly like that of the *Ley-* " *den* experiment. With this plate of air, as we may call " it, they made a variety of experiments. The two me- " tal plates, being in oppoſite ſtates, ſtrongly attracted one " another, and would have ruſhed together if they had " not been kept aſunder by the ſtrings. Sometimes the " electricity of both would be diſcharged by a ſtrong ſpark " between them, as when a pane of glaſs burſts with too " great a charge. A finger put between them promoted " the diſcharge, and felt the ſhock. If an eminence was " made on either of the plates the ſelf-diſcharge would al- " ways be made through it, and a pointed body fixed up- " on either of them prevented their being charged at all."

To the foregoing relation of the experiments themſelves, I ſhall ſubjoin the concluſions drawn from them by the curious electricians who made them, in the words of doctor *Prieſtley*, viz. " The ſtate of theſe two plates, they " (*Wilkie* and *Æpinus*) " excellently obſerve, juſtly repreſents the " ſtate of the clouds and the earth" (and perhaps of different clouds at various heights one over another) " dur- " ing a thunder ſtorm; the clouds being always in one " ſtate, and the earth in the oppoſite; while the body of " air between them anſwers the ſame purpoſe as the ſmall " plate of air between the boards, or the plate of glaſs be- " tween the two metal coatings in the *Leyden* experiment. " The phenomenon of lightening is the burſting of the " plate of air by a ſpontaneous diſcharge, which is always " made through eminencies, and the bodies through which " the diſcharge is made are violently ſhocked."

As

As in the former experiment made with the pane of glaſs, the charges, both poſitive and negative, reſide in the glaſs itſelf, and not in the coatings, thoſe remaining after theſe are removed; ſo in the latter, which is completely analogous to it, the charges are accumulated and reſide in the air ſituated between the boards, and not in their tin linings, which ſerve only as conductors, to diſtribute the fluid equally over, or to convey it equally from, the whole ſurface of air which is limited by, and in contact with them, on either ſide; whereby the whole of each ſurface may be equally charged at the ſame time, or diſcharged by the ſame exploſion.

If two or more regions of the atmoſphere, when free from vapors, become thus differently electrical in their ſtate and capacities, which, that they may, from the heat and conſequent rarefaction in a ſummer's day, we have already ſeen, and perhaps from a variety of other cauſes to us unknown; and if from the contrary currents of air which frequently take place at different heights, they ſhould perchance become ſituated one over or adjacent to another, like ſtrata of minerals within the bowels of the earth; what the metalline coating is to the pane of glaſs, or the tinned boards to the plate of air in the laſt experiment, the ſame would clouds, formed and floating therein, be to theſe regions of air; the electric equilibrium between which might be reſtored through their intervention, either by ſpontaneous diſcharges through the pure air between them in ſevere flaſhes of lightening or through the falling drops of rain, which in their ſucceſſive deſcent form a chain of natural conductors between one region of the air and another, and betwixt each of them and the earth; the paſſage of the electric fluid through which would alſo be attended with lightening and thunder, but not ſo ſevere as when the diſcharge is made through the pure air; the moſt fatal lightening uſually preceding the fall of the rain.

It is not uncommon, during the riſe and progreſs of a thunder ſtorm, to ſee different ſets of clouds, at various heights in the atmoſphere, moving promiſcuouſly in all directions, as though they were impelled hither and thither by contending winds; when probably the whole phenomenon ariſes from the different electrical ſtates of the regions of the air in which they float; as they approach one or other of which, they are attracted or repelled, and move accordingly, communicating, receiving, or tranſmitting the electric fluid, to or from them reſpectively, as they may be either deficient of their natural quantity, or poſſeſs a redundancy of this fluid. And as in the experiment of Meſſrs *Wilkie* and *Æpinus* mentioned above, the two tin plates with the boards they covered, would have ruſhed together had they not been kept aſunder by the ſtrings, ſo theſe clouds floating freely in air, and being at liberty to act upon every impulſe, gradually coaleſce, reſtoring the electric equilibrium to the neighbouring atmoſphere by repeated diſcharges as they unite*; till at length they form one denſe maſs of humid vapors, which precipitating in a heavy ſhower of rain, refreſh the thirſty ſoil, leaving the atmoſphere above in a homogenous electric ſtate, calm and ſerene.

How theſe clouds are generated, formed, and adapted to thoſe grand purpoſes in the œconomy of nature, is next to be conſidered: In proſecution of which inquiries I ſhall ſubmit the following obſervations to the candor of the reader.

Whatever the immediate cauſe of evaporation may be, it is certain that the ſuperficial moiſture of all bodies is perpetually exhaling in vapors, which aſcend into the higher regions of the atmoſphere, where they gather and are formed into clouds, and at length recondenſe, deſcending

* It is certain that in moſt thunder ſtorms the flaſhes of lightening are chiefly diſcharged from cloud to cloud, very few, and frequently none at all taking place between the cloud and the earth.

ing in dew, miſt or rain upon the ſurface of the earth from whence they ſprang.

Theſe vapors are either detached in ſtreams from the humid ground by the influence of the ſun, or thrown off by the perſpirations of thoſe infinite multitudes of animals and plants which cover the face of the earth*, or ſupplied by evaporation, from the ocean, or other grand collections of water.

Ignorant as we are of the nature of theſe operations, and of the manner in which they are performed, it is natural to ſuppoſe, that the vapors themſelves aſcend in the ſame electric ſtate, whether poſitive, neutral or negative, with the ſubſtances from which they ariſe. Accordingly ſignior *Beccaria*, in making ſome of his experiments, obſerved, that "ſteam riſing from an electrified eolipile diffuſes it-"ſelf with the ſame uniformity with which thunder clouds "ſpread themſelves and ſwell into arches, extending itſelf "towards any conducting ſubſtance†." This ſtream then was electrified as well as the eolipile from whence it proceeded. The ſea muſt neceſſarily be ſuppoſed, in common with the whole terraqueous maſs, to contain juſt its natural quantity of the electric fluid, and no more: We may therefore conclude that both the vapors which ariſe immediately from it, and the air which ſuſtains them, and from its ſituation enjoys a more equable temperature, than that over the land, are in the ſame electrical ſtate with the ſea itſelf, containing neither *more* nor *leſs* than their *natural quantity*.

Conſidering the vaſt extent of the ocean, and the comparatively ſmall degree of moiſture of which the dry land is ſuſceptible, we may conclude, that a very ſmall proportion of the clouds which are formed in the atmoſphere are exhaled from the latter, and that the ocean is the grand ſource from whence they principally derive their origin.

* See *Hales's* vegetable ſtatics, and Chambers's *cycloped*, under the word, Perſpiration.
† Prieſtley's Hiſtory, page 327.

Our ſenſes accordingly convince us that the ſea-air is always replete with moiſt vapors, even when its natural tranſparency is not in the leaſt interrupted by them. Hence in a hot ſummer's day, when the wind ſuddenly ſhifts from weſt to eaſt, we immediately perceive a chill from the ſea-breeze; and ſometimes long before the thermometer indicates a change in the temperature of the atmoſphere. Theſe vapors, when they firſt ariſe from the ſea, are generally ſo nearly of the ſame denſity with the ſurrounding and contiguous air, that the rays of light in paſſing through them, undergo no ſenſible change in their refraction; they are therefore at firſt generally inviſible, but when the weather is extreamly cold, and the air of conſequence uncommonly denſe, they are always viſible, and appear like a ſteam ariſing from boiling water*. Not that vapors aſcend moſt copiouſly in the coldeſt ſeaſons, which ſeems contrary both to reaſon and experience; but that the different denſities of the air next the ſurface of the water, and of the vapors which aſcend in it, render the latter viſible, by the irregular refractions of the rays of light in paſſing through them. For the ſame reaſon our breath is viſible in the winter, but not in warm weather.

Let us now ſuppoſe the atmoſphere, on a ſummer's morning, to be all around in a homogenous ſtate, as indicated by a cloudleſs ſky and a dead calm. As the ſun riſes on the eaſtern coaſts of America, and warms and rarefies the atmoſphere eaſtward, the rarefied air naturally aſcends, and a current of air as naturally flows thither from the oppoſite quarter, which is but juſt emerging from the cool ſhades of night, to ſupply its place. The conſequence of which is a light weſterly breeze. As the ſun aſcends higher, the air over the land becomes heated and rarefied, both by the paſſage of the ſun's direct and reflected rays through it, and by the reverberation of the heat acquired from

* This is always the appearance in a clear, ſtill morning, when the mercury in Farenheit's thermometer is at 0, or below it.

from them by the ſurface of the earth; till at length that whole region of the atmoſphere has its electrical capacity enlarged, thereby becoming negatively electriſed, or in a craving ſtate, as obſerved before. On the contrary the ſun's rays which fall upon the ſurface of the ſea, eſpecially when ruffled by wind, chiefly enter that tranſparent medium, in which they are refracted and irrecoverably abſorbed; very few, comparatively, being reflected; whence very little heat can be reverberated from that element to warm the incumbent air, which is ſenſibly affected only by the paſſage of the ſun's direct rays through it, unleſs the weather be calm and the ſurface very ſmooth*. Beſides, it is colder at ſea than aſhore in the ſummer ſeaſon, when, and when only thunder ſhowers are frequent, and indeed warmer in the winter, for the following reaſon, viz. as the ſea is every moment changing its ſurface, neither heat nor cold can affect it ſo ſoon as they do the ſurface of the earth, which continues the ſame.

The air over the land, when thoroughly heated and rarefied, naturally aſcends into the higher regions, while the denſer air from the ſea neceſſarily flows in and takes its place. Hence, probably, the eaſterly winds which uſually ſpring up near the middle of the day, after a ſultry morning.

This body of warm air aſcends till it arrives at that region of the atmoſphere in which thunder clouds are formed; while the vapors which are wafted to the continent by the eaſtern current, being attracted by this now ſuperior air which demands a ſupply of the electric fluid, con-

 tinually

* In a perfect calm the ſurface of the ſea acts like a mirror upon the ſun's rays, ſtrongly reverberating them back into the atmoſphere, *when* the heat is as ſenſible upon water as upon the dry land. But whenever that ſurface becomes agitated and broken by the force of wind acting upon it, thoſe rays, by perpetually impinging upon an infinite variety of new formed, fluctuating ſurfaces undergo innumerable refractions, in all directions, whereby they are abſorbed and loſt within the fluid maſs in ſome proportion to the violence of the agitation. Accordingly when the weather is ſerene and calm, the ſurface like a looking-glaſs reflects the phenomena of the ſky over head; upon the firſt ſpringing up of a breeze it changes to a light blue, which deepens to a fine ſky-blue as the wind riſes, to a deeper ſea-green in a briſk gale, and to a ſullen blackneſs in a ſtorm, excepting where the waves are interſperſed with white heads of foam, which, by contraſt, only render the ſcene more gloomy.

tinually aſcend till they arrive at it, leaving the denſer air, with which they were firſt connected, behind. As theſe vapors move freely through and mix with air, they eaſily inſinuate themſelves between the particles of that fluid, and unite with it, whereby every particle of air which, from the cauſes aforeſaid, is become in any degree deſtitute of the quantity of electric matter which is natural to it in its preſent ſtate, may and will attract and attach to itſelf one or more particles of this vapor, and thereby furniſh itſelf with a non-electric coating, and thus become qualified to receive from any neighbouring object ſuch a ſupply of the electric fluid as its ſtate may demand.

Thus provided, this body of air, together with the vapors which are more or leſs attached to every particle of it, will conſtitute a denſe cloud; and as the air itſelf was before (by ſuppoſition) in a craving or negative ſtate of electricity; and as the vapors are preſumed to have ariſen from the ocean in their natural or neutral ſtate, the whole body of a cloud formed by them will ſtill be in a negative ſtate, and thereby conſtitute a complete thunder cloud; which when formed, if uniform in denſity and contexture, ſhould it be attracted within the *ſtriking diſtance* from any object ſtanding upon the earth, would have its electric equilibrium reſtored at once by a flaſh of lightening darting from the earth: Or ſhould it paſs near another cloud in a different ſtate, the flaſh would reſtore an equilibrium between the two clouds.

That a body of air, either in a poſitive or negative ſtate of electricity, while pure, ſhould be incapable of communicating its ſurpluſage of the electric element to, or receiving ſupplies from the neighbouring regions, though in a contrary ſtate; and that the ſame air, when replete with watery vapors, may be reſtored to an equilibrium throughout its whole extent by an inſtantaneous diſcharge, may yet require ſome further evidence before it be admitted.

But,

But, as the particles both of air and vapor are ſeverally too minute to fall under our notice, I ſhall endeavour to illuſtrate by analogy what cannot be directly demonſtrated by experiment. In order to this, I ſhall firſt give a general deſcription of, and then ſubjoin ſome obſervations upon doctor *Prieſtley*'s electrical battery.

This battery conſiſted of ſixty four cylindrical glaſs jars fixed in a ſquare box; the jars were coated within and without with tin foil, and the floor of the box was covered with the ſame, whereby the outſides of all the jars formed but one continued electrical ſurface. In like manner, by means of ſmall braſs bars extending over the mouths of the jars in their ſeveral ranges, and by wires which connected the ſeveral bars, together with others which deſcended from them, communicating with the inner coating of each jar, their interior ſurfaces were ſo connected as to form, in the ſame ſenſe, but one ſurface. Thus conſtructed, the whole battery is capable of being equally charged in every part at the ſame time, and of being diſcharged throughout by the ſame exploſion.

Here I would obſerve, that if, inſtead of the metalline coatings, the jars were filled with water to the ſame height with them, and were immerſed in the ſame order in a ſquare veſſel of water to an equal depth, the bars and wire remaining as before, the ſucceſs of all the experiments made with them would be the ſame as above. Let then a battery be conſtructed and charged in this form; after which let the bars and wires aforeſaid be removed, and the water contained in the jars be decanted off by glaſs ſyphons, and let the water be drawn off from the veſſel in which they ſtand. It is evident from the experiment of the charged pane of glaſs already mentioned, and other experiments recited in doctor Franklin's letters, that theſe jars will remain *ſeverally* charged, as they were *jointly* before. They may now, when dry, be taken out and handled at pleaſure with ſafety; nor can they be eaſily re-

ſtored

ſtored to their natural ſtates, but either by immerſing them ſingly under water, or by replacing the whole apparatus and filling both the jars, and the box which contains them, with water as at firſt, and introducing a metalline conductor betwixt the water without the jars and any one of the wires which connect their inſides; then the whole will be inſtantly diſcharged with an exploſion*.

To apply theſe obſervations to the preſent ſubject, we may regard every particle of a body of pure†, but incidentally electrified air, in the ſame light with one of the jars in the battery aforeſaid, which, after having been charged, is deprived of its adventitious coatings: Each particle, like one of thoſe jars, will retain the ſtate it may happen to be in, ſo long as it remains deſtitute of a conducting appendage. But when, and by what means ſoever, a ſufficiency of moiſt vapors ſhall become interſperſed amongſt theſe particles of air to furniſh them ſeverally with non-electric coatings, and by the nearneſs or contiguity of theſe vapors to form a communication from one to another throughout the whole, they will then be in the ſame connected ſtate with the jars in the battery, when complete in every part, and charged; and like thoſe jars be the particles ever ſo numerous, they will be in a capacity of jointly receiving or communicating the electric fire. And as, by the addition of jars in the conſtruction of the battery, the exploſion at the diſcharge may be increaſed indefinitely, ſo will the violence of the exploſion from a thunder cloud be increaſed in proportion to its extent, and to the multitude of aerial particles together with their appendant vapors of which it conſiſts, and which are ſo connected as to be capable of uniting in the ſame diſcharge. But as a thunder cloud is not uſually formed at once, but by degrees, ſmaller clouds generally forming themſelves in

* Theſe experiments I never ſaw particularly made, but the concluſions neceſſarily follow from ſome which I have ſeen, as well as from thoſe pointed out above.

† Pure as to the purpoſes of electricity, or free from conducting vapors; perhaps pure elementary air is not to be found in our atmoſphere.

in ſeparate parties before they join the main body; and as the electrical ſtates of theſe clouds may be very different from each other, from the different electrical ſtates of thoſe parts of the atmoſphere in which they gather; the general equilibrium of the atmoſphere over a country cannot be reſtored by a ſingle diſcharge, but ſucceſſive flaſhes will dart from cloud to cloud, and betwixt theſe and the earth, till at length the whole collected maſs of vapor is ſpent and diſſolved in rain.

Here a common obſervation naturally occurs, viz. that frequently after a flaſh of lightening a ſudden ſhower deſcends in large drops. The mutual attraction between the vapors and the air, when in this electrical ſtate, is ſufficient to ſuſtain the former, notwithſtanding that they are by this attraction greatly condenſed, being as it were forced into a phyſical contact, both with the particles of air, and with each other*. But as ſoon as the air is reſtored to its natural electric ſtate by a flaſh of lightening, this attraction ceaſes, and the vapors precipitate by their own ſpecific gravity in a heavy ſhower.

Long and extenſive calms, in certain latitudes and ſeaſons, take place upon the ocean, during the continuance of which, the heat is ſcarcely tolerable. (See note, page 91.) Where theſe take place the air will naturally undergo the ſame changes, in its denſity and electric capacity, as the air over the land does in the ſummer ſeaſon, and, when ſufficiently

* A gentleman of my acquaintance, who is both intelligent and curious, informed me ſome years ſince, that he was once upon the top of a mountain in Spain, upon which a thunder cloud gathered; that as ſoon as the cloud became inſulated from the mountain it diſcharged a violent tempeſt of thunder and lightening upon the plains below; that he never was ſo thoroughly ſoaked in the moſt violent ſhower as when in the body of this cloud, though without a drop of rain, feeling as if he had been immerſed in a river. This idea is further juſtified by the ſolid appearance of the clouds that riſe in the weſt on a hot ſummer's day, compared with thoſe which float in the atmoſphere at other ſeaſons; which ſhews a manifeſt difference in their denſity and contexture: And when we obſerve attentively the ſeveral parts of a thunder cloud, the diſtinctneſs of their borders and their ſwelling furbeloes; how ſtrongly they reflect the rays of the ſun, thereby exhibiting the moſt vivid lights and deep contraſting ſhades; and on the other hand obſerve the beautiful effects of their refractive power in the intenſe golden ſkirts which adorn the riſing cloud with a fetting ſun behind it; we muſt neceſſarily conclude, that, although the vapors of which ſuch clouds conſiſt are collected and condenſed in higher regions of the atmoſphere than are thoſe which uſually form clouds at other ſeaſons, yet their denſity and ſpecific gravity is much greater; and they derive their ſupport from the electric principle.

ſufficiently heated and rarefied, will in like manner aſcend, its place being ſupplied by the denſer air from all quarters without the limits of the calm. This heated and conſequently (granting the principles of the preſent theory) electrical air, when raiſed to a certain height in the atmoſphere, may become as well adapted to the formation of a thunder cloud, from the vapors which are perpetually exhaling from the ſea, as the air over the land under the like circumſtances. Wherefore, in ſome latitudes in all ſeaſons, and perhaps in all latitudes in different ſeaſons of the year, thunder ſtorms may as well happen at ſea, even at remote diſtances from land, as aſhore.

I now proceed to conſider an objection which may be raiſed againſt the foregoing theory, which I ſhall firſt ſtate in its full force, and then endeavour to give a ſatisfactory anſwer to it.

Objection. If the electrification of that body of air in which a thunder cloud is formed depends upon the *heat* it has previouſly acquired, whence is it that thunder ſtorms are frequently attended with ſhowers of hail, which hail is ſometimes ſo large as to indicate its deſcent from the coldeſt regions of the atmoſphere?

Anſwer. Sir *Iſaac Newton* aſſerts from experiments of his own, that "the denſity of the air in the atmoſphere of "the earth is as the weight of the whole *incumbent* air." Conſequently the air gradually decreaſes in denſity from the ſurface of the earth to the top of the atmoſphere. The body of air which is ſuppoſed in this *theory* to be qualified by the action of heat upon it, to become a proper *ſubſtratum* for the formation and ſupport of a thunder cloud, is thereby expanded and rarefied, and thence becomes ſpecifically higher than it was before: It therefore aſcends till it arrives at that height in the atmoſphere at which the air is naturally, from its ſituation, of the ſame rarety with itſelf; and there it reſts in equilibrio. This region is extreamly cold at all ſeaſons, as appears from the teſtimonies of

of travellers who have viſited the tops of very high mountains, even under the line. The greater the heat which this body of air acquires below, the greater degree of rarefaction it undergoes, and the higher, of conſequence, it aſcends in the atmoſphere, where the cold is proportionably more ſevere than is uſual near the ſurface of the earth. But though it was the heat which it acquired below that firſt rarefied and expanded it, it will by no means be proportionably recondenſed by the cold which it meets with in its aſcent; for as the heat which occaſioned its rarefaction decreaſes upon that account, the preſſure of the incumbent atmoſphere upon it decreaſes as it riſes, whereby its denſity may, upon the whole, remain nearly the ſame; if ſo, may we not ſuppoſe its electrical ſtate alſo, previous to the formation of the cloud, to continue nearly the ſame? For ſhould this warm air aſcend all together as in a body, without intermixing with the denſer ſurrounding air through which it riſes, as a bubble of air does in any other fluid, and as *this* air probably would in a calm ſeaſon, the denſer parts of the atmoſphere eaſily giving way to it, till it arrives at that region the denſity of which is equal to its own, where it would be at reſt; ſhould this, I ſay, be the caſe, it would not, even in that cold region, cool ſo ſuddenly as to undergo any immediate change in its electrical ſtate, from the natural coldneſs of the region; neither would it be from condenſation, its denſity remaining nearly the ſame, as obſerved above.

But when the cloud is formed, or rather when a number of clouds are forming in the neighbourhood of each other, and joining their forces preparatory to the tempeſt, a general confuſion takes place in the atmoſphere; various and even contrary currents of air flowing promiſcuouſly hither and thither, as is evident from the viſible irregular motions of detached parts of the clouds. In this general effort of nature to reſtore an equilibrium, ſome of theſe aerial currents will probably introduce air, which having been

till now at a diſtance from the ſcene of action, has ſuffered no material change in its *natural* electric ſtate*; and is on the contrary fraught with all the cold which is natural to the region of the atmoſphere from whence it came. In falling through this adventitious current of air, the drops of rain, precipitating from the body of clouds above, are congealed into ice, and deſcend in hail, which as it falls collects other ſnowy or icy particles round it; a hail-ſtone when it comes to the ground reſembling denſe ſnow with a nucleus or kernel of ſolid ice in the middle.

That the air which this hail-ſtone falls through is colder than the region from whence it deſcends, may be thus proved, viz. If the freezing took place where, and as ſoon as the vapors were firſt ſet at liberty by a flaſh of lightening, it would be impoſſible for them ever to unite into drops, but they muſt deſcend in the fineſt chryſtals, an aſſemblage of which conſtitutes a flake of ſnow; the nucleus, or proper hail-ſtone then muſt have been firſt a fluid drop, and afterwards congealed in its fall through a colder region than that in which it was formed.

It may be further objected, that a thunder cloud, in the eaſtern parts of America, always makes its firſt appearance in the weſt, over the land, its progreſs being *towards* the ſea; which ſeems to contradict the ſuppoſition in the theory, that the vapors of which it conſiſts are chiefly ſupplied *from* the ſea.

To which I anſwer, 1. That a thunder cloud is with us very rarely, indeed ſcarcely ever formed in the weſt, without a ſea-breeze ſpringing up previouſly from the eaſt, 2. That the ſea air, as obſerved before, always abounds with vapors, although from the cauſes already aſſigned, they are uſually, at their firſt riſing, inviſible. 3. That the firſt appearance of a cloud will always be where the vapors are

* This ſuppoſition will be juſtified by conſidering, that ſuch is frequently the ſtate of the atmoſphere, that the thunder clouds which are formed in it are but of ſmall extent; notwithſtanding which, the change in the ſtate of the air occaſioned by them is perceived to the diſtance of many leagues round.

are firſt collected into a body and condenſed, and thereby rendered viſible, which in a thunder cloud will be in the weſt, notwithſtanding the vapors of which it conſiſts may chiefly have ariſen from the ſea. 4. That when a thunder cloud is once formed it will be in a ſtate of attraction with the earth in general, and more eſpecially ſo with all ſubſtances which are natural conductors of the electric fluid, ſuch as the water contained in rivers, bays, arms of the ſea, &c. and by theſe the courſe of a thunder cloud is known to be very ſenſibly affected.

But the ocean is the grand object towards which its courſe will be directed; accordingly the progreſs of the clouds is from the weſtern horizon, eaſtward, be the weather below what it may, not excepting the moſt violent eaſterly ſtorms, which are ſometimes, though but rarely, accompanied with thunder and lightening.

To the foregoing obſervations I would add, 5. That when an extenſive thunder cloud is forming in the atmoſphere by means of the mutual attraction of the condenſing vapors, and the body of electriſed air which ſuſtains and condenſes them, the increaſing denſity of the whole compound maſs of air and vapor will, by degrees, occaſion its redeſcent towards the earth, from the law of gravity; it will alſo be attracted by, and move towards the ocean, upon the principles of electricity; the cloud will then deſcend obliquely, in a diagonal between the directions of theſe two powers; and both, continually acting upon it, will jointly accelerate its motion. Such a cloud, if denſe and large, would end in a perfect tornado, either upon the land or water, as thunder ſhowers frequently do; ſmaller clouds being alſo, uſually, accompanied with guſts or flurries of wind.

I ſhall here add one obſervation more which I have frequently made, and which may tend to confirm the foregoing theory, viz. That as the general courſe of the eaſtern coaſt of north America is from north-eaſt to ſouth-weſt;

the courſe of a thunder cloud is uſually from the north-weſt, with the wind at ſouth-eaſt, perpendicular to the direction of the coaſt, and contrary to each other.

Inland ſeas and great lakes, ſuch as are thoſe in North-America, may anſwer the ſame purpoſes in the interior parts of the country, as the ocean does near the limits of the continent; both by affording the neceſſary ſupplies of vapors for the formation of the clouds, and by their attractive influence upon thoſe clouds when formed.

I now conclude with a few hints, which I ſhall throw into the form of queries.

1. Whatever the primary cauſe of evaporation may be, does not the formation of vapors into diſtinct clouds depend upon the electrical ſtate of the atmoſphere?

2. Were the atmoſphere always uniformly electrical could we have any rain*; in that caſe, if evaporation be performed independent of electricity, ſhould we not be invelloped in everlaſting fogs?

3. Mr. *Canton* ſuppoſes that the *aurora borealis* may be "the flaſhing of electric fire from poſitive towards nega-"tive clouds, throughout the upper part of the atmoſ-"phere." But as the air is uſually charged more or leſs with vapors, even when perfectly pellucid; and as the moſt remarkable *auroræ* frequently appear without a cloud in the hemiſphere, may not this phenomenon be rather occaſioned by the "flaſhing of electric fire," from one region or body of air to another in a different ſtate of electricity, through the intervening vapors?

4. May not the reaſon of its uſual appearance in the north and of its flaſhing ſouthward be, that, in every northern latitude, the air to the ſouthward is at all ſeaſons of the year, *cæteris paribus*, more affected by the heat of the ſun than the air northward of the ſame latitude; and does not this occaſion an electrical current to flow from north to ſouth,

* Signior *Beccaria* concludes from experiments, that gentle rains are the effects of a moderate, as thunder ſhowers are of a more plentiful, electricity.

ſouth, ſo often as the above mentioned circumſtances concur, though with ſome interruption from the irregular diſpoſition of the conducting vapors; and may not this occaſion thoſe gleams and ſtreams with which this phenomenon is uſually attended?

N° VIII.

Theory of Water Spouts, by ANDREW OLIVER, *Eſquire, of Salem in the State of Maſſachuſetts.*

IN my laſt I took the liberty to communicate to the Philoſophical Society a Theory of *Lightening* and *Thunder Storms*, which was ſuggeſted to my mind upon the peruſal of doctor *Prieſtley's hiſtory of electricity*. In the inveſtigation of which theory, while I was endeavouring to account for the exhibitions of thoſe phenomena upon the ocean, at great diſtances from the land, ſome thoughts naturally occurred relative to the *water ſpout;* a phenomenon as curious perhaps as any one in nature, and which can rarely take place but at ſea.

WATER SPOUTS have by ſome been ſuppoſed to be merely electrical in their origin; particularly by ſignior *Beccaria*, (*Prieſtley*'s hiſt. of elect. p. 355, 356) who ſeems to have ſupported his hypotheſis by ſome experiments. But as ſeveral ſucceſſive phenomena are neceſſary to conſtitute a complete water ſpout, (ſome of which undoubtedly depend upon the electric principle) if we attend to the moſt authentic deſcriptions of theſe ſpouts, through their various ſtages, from their firſt exhibition to their total diſſipation, we ſhall be obliged to have recourſe to ſome other principle, in order to obtain a complete ſolution. I ſhall therefore, *firſt*, deſcribe theſe phenomena according to the beſt obſervations I have met with; and *then*, endeavour to give

give a general philoſophical ſolution of them. But I muſt here obſerve, that the following deſcriptions are all taken from the accounts of mariners, who are indeed the only perſons that have opportunities of viewing them; but, unfortunately for the cauſe of philoſophy, do not uſually obſerve them with that circumſtantial accuracy, reſpecting the previous and ſubſequent ſtates of the atmoſphere, which may be neceſſary to found a complete phyſical ſolution upon; nor with any view to that end, as it is foreign to their main buſineſs, trade and commerce. But as ſuch accounts are the beſt I have met with even in the Tranſactions of the Royal Society down to 1744, lower than which I have not ſeen them; from ſuch I ſhall endeavour to draw the beſt concluſion which the nature of the evidence will juſtify.

The moſt intelligent and beautiful account of a water ſpout that I ever met with, is in the abridgment of the Phil. Tranſ. vol. VIII, by *Martin*, pa. 655, as it was obſerved by Mr. *Joſeph Harris*, May 21, 1732, about ſunſet, lat. 32° 30′ N. long. 9° E. from cape Florida; which I ſhall here tranſcribe.

"When firſt we ſaw the ſpout (ſays he) it was whole "and entire, and much of the ſhape and proportion of a "ſpeaking trumpet; the ſmall end being downwards, and "reaching to the ſea, and the big end terminated in a black "thick cloud. The ſpout itſelf was very black, and the "more ſo the higher up. It ſeemed to be exactly perpen-"dicular to the horizon, and its ſides perfectly ſmooth, "without the leaſt ruggedneſs. Where it fell the ſpray "of the ſea roſe to a conſiderable height, which made "ſomewhat the appearance of a great ſmoke. From the "firſt time we ſaw it, it continued whole about a minute, "and till it was quite diſſipated about three minutes. It "began to waſte from below, and ſo gradually up, while "the upper part remained entire, without any viſible al-"teration, till at laſt it ended in the black cloud above.

"Upon

" Upon which there ſeemed to fall a very heavy rain in " that neighbourhood. There was but little wind, and " the ſky elſewhere was pretty ſerene——"

In other accounts contained in the Philoſophical Tranſactions, theſe phenomena are deſcribed as having the appearance of a ſword pointing downwards, ſometimes perpendicularly ſometimes obliquely towards a column of water or froth, which ſeems to riſe out of the ſea to meet it, attended with a violent ebullition or perturbation at the ſurface. Again in others the appearance is compared to ſmoke aſcending viſibly as through the funnel of a chimney, either directly, or with a ſpiral motion, which according to the fancies of ſome reſembles the aſcent of water in the ſcrew of *Archimedes;* by ſuppoſing ſomething ſimilar to which in the atmoſphere, they have endeavoured to account for the riſe of the water from the ſea in a water-ſpout. To which I would add, that, from the relations of ſome perſons who uſe the ſea, with whom I have converſed upon the ſubject, I find that it is no uncommon thing, during a calm below, and a ſerene ſky above, to obſerve at the diſtance of two or three leagues a ſmall cloud hovering in the air, from whence the commencing ſpout ſeems to dart downward to the ſea, upon which the uſual phenomena take place in their order. I have alſo been informed (and to information I muſt truſt, having never been at ſea) that it is common during theſe appearances for ſhips to ſail, even within hail of each other, with different winds; and within the limits of the ſame viſible horizon, with contrary winds: And laſtly, that the riſe and progreſs of this phenomenon is ſometimes ſo rapid, that, even in a ſerene ſky, a few minutes will be ſufficient to generate a cloud from one of theſe ſpouts, and to diſcharge from thence a heavy ſhower of rain.

Before I proceed to attempt a philoſophical ſolution of theſe curious productions of nature, in which the two principal fluids of our globe, air and water, are largely concerned;

concerned; it may be neceſſary to make ſome obſervations upon the nature and properties of fluids in general, as ſuch.

1. No fluid can be at reſt unleſs every part of it reſpectively be acted upon by an equal force or preſſure in every direction, till when its ſeveral parts will neceſſarily recede from the greater preſſure towards the leſſer, nor can an equilibrium take place.

2. If two or more fluids of different natures and denſities come together, ſuch as quickſilver, water, oil and air, which will not mix; they will take their places according to their ſpecific gravities, the denſeſt remaining at the bottom.

3. If a veſſel be filled with either of theſe fluids, and a denſer be admitted into it, the latter will expel, and take place of the former.

4. If an empty cylindrical ſpace be ſurrounded on all ſides by a fluid, which is excluded by ſome reſiſting ſurface terminating that ſpace, the fluid will neceſſarily, upon the ſudden removal of the obſtacle, immediately flow in from every ſide towards the center of the void; and as it flows inwards the parts next ſurrounding this ſpace will thereby be crowded together, and force each other upwards, till at length when cloſed, the fluid will by its aſcent have formed a column directly over the middle of the ſpace, to a height proportionable to the united force of the converging currents. This muſt be the caſe with every fluid thus flowing into a vacuum; and in a leſſer degree when a denſer fluid in a ſimilar ſituation ſupplants a rarer: And the greater the difference of the denſities of the two fluids might be, the more conſpicuous would be the effect.

This reaſoning may be illuſtrated, and the concluſions exemplified by facts which muſt have occurred to the obſervation of every one. Do we not obſerve when a ſhower of hail, or rain in large drops, falls upon the ſurface of ſtagnant water, that the water riſes wherever they fall, like ſo many little inverted icicles, which again inſtantly

inſtantly ſubſide? The cauſe of which undoubtedly is, that theſe drops, or hail-ſtones, deſcending from a great height in the atmoſphere, acquire ſeverally ſuch a momentum in their fall as to plunge through the ſurface to a proportional depth, driving the ſuperficial water back on every ſide, and leaving a momentary vacuum behind them; not indeed a pure vacuum, but ſuch, relative to the ſurrounding fluid, which immediately returns to fill up the chaſm, and, as it cloſes, gathers and riſes in the little columns above deſcribed. When a large round ſtone, or any other heavy body plunges, the effect is proportionably greater.

5. Let us, for argument's ſake, ſuppoſe the atmoſphere over any certain circular tract of ocean of ſome miles in diameter, to be for a moment annihilated, the ſpace it occupied before being reduced to a pure vacuum: The ſurrounding atmoſphere, when at liberty, would ruſh in from every quarter towards the centre, where the converging currents would immenſely croud each other, and force up a vaſt quantity of air through a very narrow funnel, contracted below by the united preſſure of thoſe currents from all ſides, into the higher regions; which funnel, as the denſity of the air leſſens according to its height, and the ſurrounding preſſure which contracts it muſt decreaſe nearly in the ſame proportion, would more and more diverge and expand the higher it roſe above the ſurface of the ſea. This would be attended with a moſt furious blaſt of wind up to, and far above the top of the atmoſphere. In like manner,

6. If inſtead of a pure vacuum, or a total annihilation of ſuch part of the atmoſphere, we ſuppoſe the ſame to become, by any means whatever, ſpecifically lihgter than the ſurrounding regions, the effect would be the ſame as above, in kind, though not in degree; the denſer air flowing in, but with leſs rapidity, from all quarters without, expelling the lighter and ſupplying its place, as in article four; upon which alſo a large quantity of this confluent air, for the

ſame reaſon, would be driven up with violence through a like narrow vent, yet not with the ſame impetuoſity, nor to the ſame height as if forced through this funnel into a pure vacuum.

That the atmoſphere over large tracts of ſea or land may thus become ſpecifically lighter than that over the ſurrounding regions, will be evident, if we conſider, 1. That heat has a natural tendency to rarefy and expand the air upon which it acts. 2. That the atmoſphere over our heads does not conſiſt of mere elementary air, but is an univerſal receptacle of all the heterogeneous vapors and effluvia that are perpetually exhaling from every ſubſtance that exiſts upon the face of the earth, whether animal, vegetable or mineral. 3. That, by the caſual diſpoſition of theſe vapors and effluvia in the atmoſphere, the air, which is, of itſelf, naturally enough diſpoſed to acquire heat from the paſſage of the ſun's rays through it, may become more diſpoſed to imbibe and retain that heat, in one region, than in another in its neighbourhood; which, from the intervention of clouds, or from its purity and freedom from thoſe ſteams and vapors with which the former is charged, may, in a great degree, retain its natural coolneſs and denſity, while the other becomes heated, rarefied and expanded, and is thereby rendered ſpecifically lighter.

That theſe different affections of the atmoſphere *actually* take place, and diſpoſe the air, at one time and in one place, even in the ſame ſeaſons of the year, to imbibe and retain the heat excited by the ſun's rays, more than at another, is not a matter of mere conjecture; but, whatever the cauſe may be, is notorious to all perſons of obſervation.

Theſe things being premiſed, I beg leave to obſerve further, that ſome parts of the ocean are liable to long and extenſive calms, during the continuance of which the heat is ſcarcely tolerable. Where theſe take place the air muſt neceſſarily undergo proportional changes in its denſity and

electric

*electric capacity**; and when heated and rarefied to ſome certain degree will give way, as obſerved above, to the denſer air, now proportionably diſpoſed to flow in from all quarters without the limits of the calm.

When once this ſtagnated air, eſpecially if of any great extent, becomes ſpecifically lighter than the ſurrounding air, and ſufficiently rare to be ſupplanted by it; the latter will, of courſe, ſet it from every ſide in horizontal currents; which will flow, either directly, or obliquely, towards one point, in or near the centre of the becalmed region aforeſaid; the obliquities of which currents will depend upon the directions and velocities of the winds, or currents of air which might previouſly have taken place in the ſurrounding regions. When theſe currents arrive at the centre of their mutual convergency, all the ſtagnated and rarefied air which was before incumbent upon the calm ſurface of the ſea, will have been expelled and forced higher up into the atmoſphere; upon which theſe currents, by their mutual concourſe in one place, will exceſſively croud each other, as obſerved above, wherever it happens, driving the central air upwards with a violent blaſt; which, ſhould the currents ſet in obliquely, and ſo converge with a ſpiral motion towards the centre of their mutual concourſe, would aſcend as through the ſcrew of Archimedes, or the worm of a cork-ſcrew, to both of which navigators have likened theſe ſpouts: Otherwiſe it would riſe through a ſtrait, narrow funnel, as in articles five and ſix above; which if filled with any opaque matter would become viſible, and at a diſtance would reſemble a ſpeaking trumpet with the ſmall end downwards, in which form the water ſpout frequently appears. In the former caſe a whirlwind round about the centre would undoubtedly be the conſequence; and in either, a water ſpout would probably be produced†.

* See Theory of Lightening, &c. page 81.

† We ſhall in the ſequel ſee abundant reaſon to conclude with doctor *Franklin* and others, that water ſpouts at ſea and whirlwinds on the land (ſome ſpecies of them at leaſt) are produced by the ſame cauſes.

For the preſſure of the atmoſphere is taken off from that part of the ſurface of the ſea, which is directly under the funnel through which the air is driven up; whereas the ſurrounding ſurface is at the ſame time uncommonly preſſed, from the confluence of the currents from all quarters*, whereby the water muſt neceſſarily be forced up to a certain height, proportional to the ſurrounding preſſure, through the ſame funnel with the air itſelf, nor is this all, for in their aſcent the air and water become confuſedly mixed together, whereby the latter is broken and attenuated into the fineſt globules and particles, as when one forcibly blows water out of his mouth; and from this mixture of the two fluids doubtleſs ariſes that opacity which renders the ſpout viſible.

This opaque column of air and water, together with the paſſage through which it aſcends, will expand as it riſes, in proportion as the compreſſure diminiſhes; and, to ſpectators at too great a diſtance to diſcern the narrow ſtem next the water, will reſemble a ſword, or acute cone pointing downwards from a ſmall cloud; to which they are frequently likened. But that they do at the ſame time communicate with the ſea is evident from the perturbation of the water directly under them, which ſometimes boils and foams at a great rate. This is uſually the firſt appearance of one of theſe ſpouts, the duration of which is either longer or ſhorter, and the ſubſequent phenomena more or leſs conſiderable, according to the extent of the cauſe, and the mode of its operation.

The water being thus raiſed from the ſea, and forced irreſiſtably upwards in the fineſt globules by the protruding air, arrives at length at the warm electrical air † lately expelled,

* In the abridgment of Philoſophical Tranſactions, vol. II. (by Eames and Martin) page 61, at the bottom, it appears, that the meeting of two contrary currents of air or contrary winds, raiſes the mercury in the barometer near the place where it happens, which indicates an increaſe of the preſſure of the atmoſphere upon the ſurface of the earth or ſea. How much more then muſt that preſſure be increaſed, from a general confluence of the air from all quarters towards one ſpot?

† See Theory of Lightening, &c. page 90.

expelled, which was previouſly incumbent upon the calm ſurface beneath; the electric attraction of which probably aſſiſts the further aſcent of theſe particles after the firſt fury of the blaſt is ſpent. There it undergoes another operation being converted into vapor, whereby it is wholly diſcharged of the marine ſalts it carried up with it*; which are now left to ſhift for themſelves, together with innumerable other heterogeneous corpuſcles which ſucceſſively float in the atmoſphere, and which in due time, become ſeverally ſubſervient to many wiſe purpoſes in the œconomy of nature. Theſe vapors will then be greedily attached by the craving particles of this air, now deficient of its natural quantity of electric matter†, and form a denſe cloud, in like manner as thunder clouds are formed over the land; but with much greater expedition, as the ſupply of vapors is more ſudden. This cloud will then be ready in a ſhort time to diſcharge a ſhower of freſh water upon the ſea from whence it roſe, and may be attended with thunder and lightening, or not, as the air in which the cloud was formed was more or leſs electrical, or the cloud extenſive.

A previous calm may not be *neceſſary* to the production of theſe phenomena, and indeed they frequently happen without one: But, upon the ſame principle, if it be calmer where they are produced, or the ſtate of the atmoſphere there be ſuch as to diſpoſe it to acquire and retain the heat acquired from the ſun's rays, more than in the ſurrounding regions, which, as we have ſeen above, may be the caſe, the effects may be the ſame in kind, though perhaps not in degree; the moſt perfect water ſpouts probably riſing from whence there has previouſly been a dead calm, or nearly ſuch, for the foregoing reaſons.

If

* The water carried up in one of theſe ſpouts is undoubtedly ſalt when it firſt riſes from the ſea, as it aſcends in great quantities, and in a very denſe column; but it is always freſh when it deſcends again in a ſhower: It muſt therefore in the mean time have gone through a compleat natural diſtillation.

† Theory of Lightening, &c. page 92.

If there be any wind at the time of the phenomenon, the aerial funnel through which the water aſcends, inſtead of being perpendicular to the horizon, as it would be in a calm, might incline more or leſs to it, in proportion to the ſtrength or weakneſs of the prevailing current of air: Or, inſtead of continuing in one ſpot, it might have a progreſſive motion over the ſurface of the ſea, in the direction of the general current; both of which circumſtances frequently take place. In either caſe it is natural to ſuppoſe, that both air and water would aſcend ſpirally, as through the worm of a ſcrew, every current which ſets in towards the centre receiving an oblique bias from the prevailing current.

It ſometimes happens, that after the ſubſiding of a ſpout, it is ſucceeded by a ſecond, and that by a third, either in the ſame place, or at no great diſtance from it. But this alſo is analogous to what we obſerve upon the plunging of heavy bodies out of air into water. For, after the firſt ſubſiding of the ſmall column of water which is occaſioned by it, and is above reſembled to an icicle, the water again riſes and ſubſides as at firſt, though not in the ſame degree, as may be concluded from thoſe fainter concentric circles which expand from the ſame centre after the ſubſidence of the firſt column. The ſame thing which here takes place in water, may alſo take place in air, under ſimilar circumſtances.

Since writing the foregoing, while I was endeavouring to contrive ſome experiment to illuſtrate the ſubject, a very ſimple one was ſuggeſted to my mind, the ſucceſs of which I think demonſtrates the truth of the hypotheſis introduced above to account for the firſt aſcent of the water in the ſpout; the event being preciſely the ſame as was expected before hand, and as ought to have taken place upon the principles above advanced.

EXPERIMENT.

In a ſtiff paper card I made a hole juſt big enough to inſert a gooſe quill ſo as that it might be fixed perpendicularly

cularly to the plane of the card: After cutting the quill off ſquare at both ends and fixing it, I laid the card upon the mouth of a wine glaſs, filled with water to within one fifth or ſixth part of an inch from the lower orifice of the quill; then applying my mouth to the upper part, I drew out the air in the quill by a ſtrong ſuction, and in one draught of my breath drew in about a ſpoonful of the water; this by ſtronger ſuctions I was able to repeat again and again, the quill remaining as before. The water, as I expected, did not aſcend to the mouth in a ſtream, as it would have done had the quill reached below the ſurface; but broken and confuſedly mixed with the air which aſcended with it; as is above ſuppoſed to be the caſe in the aſcent of water in a ſpout at ſea.

In this experiment the ſuction occaſioned a vacuum, or at leaſt a great rarefaction of the air, within and directly under the quill; the ſurrounding air of courſe flowed in from every quarter to ſupply it, ruſhing up into the quill, and through it to the mouth; the preſſure of the atmoſphere being thereby taken off from the ſurface of the water immediately under the orifice, while the preſſure upon the ſurrounding ſurface remained, and was probably increaſed, the water was forced up together with the air as above notwithſtanding the quill had no manner of communication with the water. If the ſuction be made very ſtrong, and the quill be fixed at the diſtance of a quarter of an inch or more from the water, a conſiderable agitation and ebullition takes place in the water under it, ſimilar to that obſerved in moſt natural water ſpouts, and the paſſage of the water from the ſurface to the quill becomes very viſible.

It was hinted in a preceding note, that water ſpouts at ſea and whirlwinds at land, ſome ſpecies of them at leaſt, ariſe from the ſame cauſe, how different ſoever their apparent effects may be. This I think is made ſufficiently evident from the obſervations of a couple of land ſpouts at *Hatfield*

Hatfield in *Yorkſhire*, by Mr. *Abr. de la Pryme**, whoſe accounts of them I ſhall here tranſcribe, as the Tranſactions of the Royal Society are in the hands of but few among us, and as the facts related by him tend ſtrongly to confirm the preſent theory, however his concluſions from them may differ from it.

"On the 15th of Auguſt, 1687, (ſays he) appeared a "ſpout in the air at *Hatfield* in *Yorkſhire*; it was about a "mile off coming directly to the place where I was; I "took my proſpective glaſſes to obſerve it as well as I "could.

"The ſeaſon was very dry, the weather *extreme hot*, "and the air very cloudy; the wind aloft, and pretty "ſtrong, and (which is remarkable) blowing out of ſeve-"ral quarters at the ſame time, and filling the air here-"abouts with mighty thick and black clouds, layer upon "layer; the wind thus blowing ſoon created a great *vor-*"*tex*, *gyration* and *whirling* among the clouds; the cen-"tre of which every now and then dropt down in the "ſhape of a thick, long, black pipe, commonly called a "ſpout; in which I could diſtinctly view a motion like "that of a ſcrew, continually drawing upwards, and ſcrew-"ing up (as it were) whatever it touched. In its progreſs "it moved ſlowly over a hedge-row and grove of young "trees which it made to bend like hazle wands, in a cir-"cular motion; then going forward to a great barn it "twitched off in a minute all the thatch, and filled the "whole air therewith. Coming to a very great oak tree, "it made it bend like the foregoing trees, and broke off "one of the greateſt and ſtrongeſt branches that would "not yield to its fury, and twiſting it about, flung it to a "very conſiderable diſtance off; then coming to the place "where I ſtood, within three hundred yards of me, I be-"held this odd phenomenon, and found that it proceeded "from nothing but a *gyration of the clouds by contrary winds*

* Abridgment of Philoſophical Tranſactions, vol. IV. by Jones, page 106, 107.

" *winds meeting in a point or centre;* and where the great-
" eſt condenſation and gravitation was, falling down into
" a pipe or great tube (ſomething like the *cochlea Archi-*
" *medis*) and that in its working or whirling motion, ei-
" ther ſucks up water, or deſtroys ſhips, &c. Having tra-
" velled about a quarter of a mile farther, it diſſolved by
" the prevalency of the wind that came out of the eaſt."

The account of the other is as follows, viz. " I have
" ſeen another ſpout in the ſame place, which very much
" confirms me in my notion of the origin and nature of
" them.—The 21ſt of June, 1702, was *pretty warm;* on
" the afternoon of which day, about two of the clock, *no*
" *wind ſtirring below* though it was ſomewhat great in the
" air, the clouds began to be mightily agitated and driven
" together; whereupon they became very black, and were
" (moſt viſibly) *hurried round*, from whence proceeded a
" moſt audible whirling noiſe, like that commonly heard
" in a mill. After a while, a long tube or ſpout came
" down from the centre of the congregated clouds, in
" which was a ſwift *ſpiral motion* like that of a ſcrew, or
" the *cochlea Archimedis* when it is in motion, by which
" ſpiral nature and ſwift turning, water aſcends up into
" the one as well as into the other. It travelled ſlowly
" from weſt to north-eaſt, broke down a great oak tree or
" two, frighted ſome out of the fields, and made others
" lie down flat upon their bellies, to ſave being whirled
" about and killed by it, as they ſaw many jackdaws to
" be, that were ſuddenly caught up, carried out of ſight,
" and then caſt a great way amongſt the corn; at laſt it
" paſſed over the town of *Hatfield*, to the great terror of
" the inhabitants, filling the whole air with the thatch that
" it plucked off from ſome of the houſes; then touching
" upon a corner of the church, it tore up ſeveral ſheets of
" lead, and rolled them ſtrangely together; ſoon after
" which it diſſolved and vaniſhed without doing any fur-
" ther miſchief.

" By all the obſervations that I could make of this, and " the former, I found that had they been at ſea and joined " to the ſurface thereof, they would have carried a vaſt " quantity of water up into the clouds, and the tubes would " then have become much more ſtrong and opaque than " they were, and have continued much longer.

" It is commonly ſaid that at ſea the water collects and " bubbles up a foot or two high under theſe ſpouts before " that they be joined: But the miſtake lies in the pellu- " cidity and fineneſs of thoſe pipes, which do moſt certain- " ly touch the ſurface of the ſea before that any conſider- " able motion be made in it, and that, when the pipe be- " gins to fill with water, it then becomes opaque and " viſible."

I ſhall here make a remark or two upon the above cited author's mode of expreſſion in the foregoing accounts, which is evidently adapted to a preconceived idea of the *cochlea Archimedis*, by ſuppoſing ſomething ſimilar to which, as taking place in our atmoſphere, he is not alone in endeavouring to account for theſe phenomena. In conformity to this idea he ſpeaks of the ſpout as *drawing upwards*, and *ſcrewing up* whatever it touched; and ſuppoſes that by its *ſpiral motion* and *ſwift turning*, water aſcends in it as in the *ſcrew of Archimedes*. But this hypotheſis, however ſpecious, has been long ſince exploded as unphiloſophical.

Mr. *de la Pryme* mentions the appearance of a long black pipe which now and then dropped down from the centre of the gyrating clouds; in which pipe he diſtinctly viewed a motion like that of a ſcrew; and as ſuch he ſeems to have ſuppoſed it acted, viz. either in the manner of a cork-ſcrew upon ſolids, or as the *cochlea Archimedis* upon fluids, drawing them up into the atmoſphere. But as he himſelf afterwards, when applying his obſervations to a ſpout at ſea, very juſtly concludes that the pellucidity and fineneſs of theſe pipes over the water render them inviſible below,

" notwithſtanding

" notwithſtanding (as he conceives) that the pipes do moſt " certainly touch the ſurface of the ſea before any conſider- " able motion be made in it, and that they are then rendered " opaque and viſible when they begin to fill with water ;" might he not with equal reaſon have ſuppoſed that thoſe aerial pipes which he obſerved over the land were alſo continued from the clouds down to the ſurface of the earth, as from their effects below, one would naturally conclude they were, and that they were pellucid and inviſible ſo long as they contained nothing but air ; but that " every " now and then," when they met with any ſubſtances which might perchance paſs within the compaſs of their gyration, or which they could eaſily carry up ; ſuch as detached parts of the broken clouds ; water from ſtagnant ponds, brooks and rivers, hay, ſtubble, thatch, duſt, &c. they then become opaque and viſible, and that they appeared to dart downwards by a kind of optical deception ? For upon the foregoing principles theſe pipes of air muſt neceſſarily be broadeſt above, as we have already ſeen, and terminate in a narrow ſtem below, the broadeſt part being, at a diſtance, firſt viſible, and the ſhank ſeemingly tapering downwards to a point. It is however certain from the effects of the above mentioned ſpouts, that, whatever the appearances were *aloft*, they were all occaſioned by the ruſhing of the air upwards through a narrow paſſage, that was contracted *below*, by the concourſe and preſſure of the oppoſite currents of that fluid, and dilated above from the diminution of that preſſure.

I have reſerved for this place an account of a curious ſpout which made its appearance *anno* 1694, not at ſea, but in the harbour of *Topſham**, and at low water ; which paſſed with a ſlow progreſſive motion over both land and water; acting as a complete water ſpout over the latter, and as a whirlwind upon the former: For when it paſſed over the channel of the river, it threw up the water in a denſe

* Lowthorp's Abridgm. Phil. Tranſ. vol. II. page 104.

ſtream, as if it had been impelled through the hoſe of a fire engine, and the ſtream accordingly ended in a thick miſt, reſembling a dark ſmoke; the ſurface of the water, round about the ſpot from whence it roſe, being greatly agitated, as is uſual in thoſe phenomena. In its courſe it met with the hull of a new ſhip of about one hundred tons, which was much ſhaken by it, but received no hurt. In paſſing over the flats it took hold of a boat which was faſtened to an anchor, whirled both boat and anchor to ſome height in the air, and rent the boat " from the *head* " to the *keel*." When it reached the ſhore it lifted up another boat about ſix feet from the ground, letting it fall again upſidedown; and had a ſtrange effect upon a parcel of planks, ſome of which were raiſed up perpendicularly, and ſtood upon their ends while it paſſed along; and in its further progreſs it was attended with the uſual effects of a whirlwind, ſuch as ſtripping off, not only thatch, but ſheets of lead from the tops of houſes, and tearing off the limbs of trees. This account may tend to confirm the theory here offered, as it proves to a demonſtration, that the water ſpout therein deſcribed, was occaſioned by a previous whirlwind in the atmoſphere; which whirlwind was alſo occaſioned by the ruſhing of a large quantity of air, upwards, from all quarters near the ſurface of the earth, through a very contracted aerial paſſage, towards the top of the atmoſphere; the narrowneſs of which paſſage, as determinable from the effects obſerved in its progreſs, ſhews it to have been compreſſed upon all ſides by a general conflux of oppoſite currents of air; as the ruſhing of the air through it with ſuch violence from beneath, does, that the denſity of the fluid and the compreſſive force of the currents were greateſt there. The aſcending air carried up the water with it through the ſame paſſage; not by any mechanical operation upon it, like the action of a ſcrew of any kind; but, merely, by taking off the preſſure of the atmoſphere from the ſurface of the water directly under it;

it; whence the water muſt neceſſarily aſcend, as in any common hydraulic machine; and that with a force proportional to the preſſure of the atmoſphere upon the ſurrounding ſurface, now greatly increaſed by the confluence of thoſe currents.

Before I cloſe this ſubject, I ſhall juſt mention, without making any remarks, the effects which a whirlwind had amongſt a number of ſhocks of corn at *Warrington* in *Northamptonſhire*, Auguſt 1ſt, 1694; out of which from eighty to a hundred ſhocks were carried up into the air, a great part of them out of ſight; theſe when the fury of the blaſt was ſpent, fell down again at the diſtance of ſome miles from their own field. The account of this whirlwind immediately precedes the article laſt quoted from the *Philoſophical Tranſactions*. Should the foregoing theory be adjudged tenable, it will render very credible thoſe ſtrange accounts which we have ſometimes had, of its raining tadpoles and frogs, which have been found upon the tops of houſes after a ſhower; and even ſmall fiſhes, a ſhower of which fell at *Cranſtead* near *Wrotham* in *Kent*, *anno* 1696, on the Wedneſday before Eaſter (Lowthorp's abridgement of Philoſophical Tranſactions, vol. II. page 144.) For ſhould one of thoſe aerial pipes paſs over a frog pond, or the ſhallow parts of a fiſh pond, the ſame natural cauſe which in a ſpout at ſea, would carry up the water from the ocean, would alſo carry up the water from the ponds aforeſaid, together with the contents; whether tadpoles, frogs or fiſhes: Theſe muſt deſcend again ſomewhere; and wherever they fell, a ſhower of fiſhes, frogs or tadpoles, would be the conſequence.

Experiments

N° IX.

Experiments on Evaporation, and Meteorological Obſervations made at Bradford in New-England, in 1772, *by the Rev.* SAMUEL WILLIAMS, A. M.

IN making experiments on the quantity of water that evaporated in the year 1771, the method I uſed, was to fill the veſſel the beginning of every month: In the courſe of theſe experiments, I obſerved that in the beginning of the month when the tube was newly filled, it exhauſted much faſter than towards the latter end, when one or two inches of the water was evaporated; and that the quantity of evaporation meaſured this way, came out leſs than the quantity of rain that fell in the courſe of the year. The beginning of the year 1772, I attempted to examine this matter more carefully. With this view I made the following experiments.

EXPERIMENT I.

I procured two cylindrical veſſels of three inches diameter, and ſix deep, as much alike as they could be made: One, I filled with water as I had done in 1771, once a month; the other, with the ſame kind of water, once a week; and placed them about ſix inches apart, in ſuch a manner as to be expoſed to the wind, and ſun, but covered from the rain. The reſult was, that which was filled once a week, exhauſted about one third more than the other. In *January* and *February*, the difference was a little leſs; in *March* and *April*, it was a little more. In *May*, the laſt month in which I compared them, the evaporation from the former was 6.35 inches; from the other 4.10. By this experiment

experiment I was convinced that it never could be known with much accuracy by either of theſe methods, what quantity of water does really evaporate from the ſurface of ſeas, lakes and rivers. For in the one caſe, after about an inch is exhauſted the ſurface of the water is too much ſheltered from the wind, which greatly retards the evaporation. In the other, as the water has all the advantage of the wind, and is heated by the ſun, and atmoſphere, to a conſiderable greater degree than the water in ſeas, lakes and rivers, the quantity of evaporation comes out too much. And therefore nothing certain as to the real quantity of evaporation from watery fluids, can be determined by ſuch experiments, however carefully they may be made.

EXPERIMENT II.

To meaſure with more certainty the real quantity of evaporation, I attempted in the next place to examine what it was in fact from the ſurface of a river. This experiment was made in the following manner: I filled one of the veſſels with river water, and placed it as before. The other I fixed in the centre of a circular board of three feet diameter. This inſtrument, by means of a line faſtened to a tree on a ſmall iſland, was placed ſo as to float near the middle of *Menimack* river. To defend the tube againſt the dews and rain, a circular piece of glaſs, fifteen inches diameter, was ſupported by wires fixed to the board, eight inches above the tube; and the whole was ſo balanced by weights as to leave half an inch of the tube above the ſurface of the water. When thus afloat I filled the tube with water, propoſing to let it remain in this ſituation a week, to ſee how much would evaporate in that ſpace of time. After repeated diſappointments by the rain, wind and waves, for three months, I at laſt ſucceeded in trying the experiment from *Auguſt* 26th, to *September* 2d. During that time there was little wind, ſtill

ſtill water, no rain, nor any thing to diſturb the experiment. The event was, that at the end of the ſeventh day, the tube was exhauſted 1.15 inch. And that no water had got into the tube in that time, I was certain from this circumſtance; all that part of the ſurface of the board which was within half a foot of the tube was dry every morning and evening. In the other tube, the evaporation in the ſame time was 1.50 inch; which gives 35 decimal parts of an inch difference between the real evaporation from the ſurface of the river, and that of the water when ſuſpended in the air, as in the other veſſel. All the evaporations therefore meaſured the latter of theſe ways, ought to be diminiſhed in this proportion, to have the true quantity ſuch as it is in nature.

EXPERIMENT III.

Theſe experiments on watery fluids put me upon enquiring what the evaporation was from the ſurface of the earth. To determine this, *Sept.* 14, two days after there had been any rain, I ſunk one of the veſſels into the earth in a light ſoil, ſo as to take up all the earth contained in a ſpace equal to the contents of the veſſel. Having carefully weighed the veſſel with the earth it contained, I fixed it in the ground in a plain open field, where it was expoſed to the ſun and wind, but defended from the dew and rain, as in the former experiment. At the end of *ſeven days* I took it up, and weighing it again found it had loſt 783 grains, *troy*. The diameter of the veſſel being three inches, its ſurface expreſſed in whole numbers was equal to nine ſquare inches. Dividing the number of grains that evaporated, 783, by the number of ſquare inches contained in the ſurface of the veſſel, 9, we ſhall have 87 grains for the evaporation from one ſquare inch; and this, (aſſuming 254 grains as the weight of a cubic inch of water) will give $\frac{34}{109}$ parts of an inch, as the depth of water that paſſed off by evaporation. In the other veſſel filled with water,

water, and placed as before, the evaporation in the ſame time was exactly one inch. If this experiment may be ſuppoſed to repreſent the operations of nature, the concluſion will be, that the evaporation from the ſurface of the earth, is but little more than *one third* of what the evaporation is from the ſurface of water.

EXPERIMENT IV.

Another thing I had in view was to know what the evaporation was from plants and trees. In order to make an eſtimate of this, *Auguſt* 20, I took up four different ſorts of plants, with as much of the earth adjoining to each as wholly covered their roots. Each plant, with the earth thus about it, being ſix inches ſquare, I put into a wooden box of the ſame form and ſize. The boxes were covered with thin lead, well cemented at the joints, that nothing might evaporate that way; and had two apertures at the top; one, to admit the ſtem of the plant, the other, that the plant might be ſupplied with water, but which was kept ſtopped when not in uſe. Having taken the weight of each, I placed them in the ground that they might have the ſame degree of heat as before; leaving as much of the plant above the ſurface of the earth, as when it was in its natural ſtate. In this ſituation I added known quantities of water, aiming to put in from time to time as much as I thought they would throw off. At the end of *thirty days* I took them up, taking an account of their weight as before, and alſo that of each plant. The reſult is expreſſed in the following particulars:

The ſeveral ſorts of plants.	*Weight of the plants.* Grains.	*Water evaporated in 30 days.* Grains.
Apple tree,	23	1271
Alder tree,	30	2593
Spear mint,	22	5186
Clover,	43	1894

In this experiment, the evaporation from theſe four very ſmall plants was 10944 grains; amounting to about 43 cubic inches of water, in thirty days. The evaporation in the ſame time from the veſſel ſuſpended in the air, was 4.25 inches in depth: The quantity therefore thrown off by the plants, was more than what the evaporation would have been from a watery ſurface, of ten inches ſquare. If this way of reaſoning may be applied to fields covered with trees, graſs, and other vegetables, the inference will be, that the evaporation for ſeveral months is greater from them, than it is from equal areas of the ſurface of water.

METEOROLOGICAL OBSERVATIONS made at BRADFORD *in* 1772.

THE inſtruments uſed in the following obſervations, the times at which they were taken, and the method in which they are ſet down, were deſcribed in the paper ſent to the Society laſt year. With regard to thoſe of the preſent year the following things are to be obſerved: The *barometrical* obſervations till Nov. 6, muſt be viewed as imperfect, being taken with a barometer of too ſmall a bore. From the 6th of November to the end of the year, they are very exact; being taken by a very good barometer made by *Nairne*. In meaſuring the quantity of evaporation, I uſed a tube three inches diameter and ſix deep; which was filled once a week. The rain was meaſured by a tube of the ſame form and ſize. In all other reſpects, the ſame method was obſerved as in the meteorological obſervations of 1771.

The obſervations taken by the *barometer* and *thermometer* are ſet down in three columns; the firſt column contains the obſervations taken uſually about 6^h A. M. the ſecond at noon, and the third at 9^h P. M. The other columns give the general ſtate of the winds and weather of the day.

JANU-

JANUARY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	30,0½	30,1	30,1	18½	25	26½	W. to S. W. little.	Fair day; ſnow in the night.
2	30,1½	30,1½	30,1	27	32	30	W. little.	Fine pleaſant weather.
3	29,7½	29,5	29,4½	18	23	33	S.	Cloudy day; rain in the night.
4	29,4	29,6	29,7½	37½	40	41	S. W.	Fine pleaſant weather.
5	29,7¾	29,7½	29,8	26	24	18	N. W. ſtrong.	Clear bright day.
6	29,9½	29,9¼	30,0½	13	15½	20	N. W.	Ditto. *Aurora Borealis* in the evening.
7	30,1	30,1	30,1	15½	18½	22½	W. to S.	Cloudy, dull weather.
8	30,1	30,1	30,1½	23	30	29½	W. little.	Fine pleaſant day.
9	30,1½	30,1	30,1	27	31½	34	Ditto.	Ditto.
10	30,1½	30,1	30,1¼	28½	30	26½	Ditto.	Ditto.
11	30,1¼	30,1	30,0½	21	25	26	Ditto.	Ditto.
12	29,8	29,6½	29,4¼	26	27	32	E. freſh.	Cloudy day; ſnow in the night.
13	29,6	29,6	29,6	35	40½	34½	S. W. to W.	Flying clouds with bright intervals.
14	29,7½	29,7½	29,8	28	31½	30	W. little.	Mild pleaſant day.
15	29,8	29,7¼	29,6	23	25	25	Ditto.	Ditto.
16	29,2	29,2	29,0½	23	27	28	S. E. to W.	Snow A. M. clear P. M.
17	29,4½	29,5	29,5	21	21	20	W.	Clear bright day.
18	29,3¼	29,2	29,2	10	14	18	N.	Cloudy, with ſome ſnow.
19	29,1½	29,1	29,1	18	21	27	N. to S. W.	Cloudy dull weather.
20	29,0	28,8½	28,6½	27½	26½	20	N. to N. W.	Storm of ſnow, all day; bright evening.
21	28,9	29,1½	29,2	11½	20	19	N. W.	Fair weather.
22	29,2	29,1½	29,1	18	29½	28½	S. E.	Cloudy dull day.
23	29,0	29,0	29,0	28½	34	36½	S. little.	Cloudy, with bright intervals.
24	29,0½	29,0¼	29,0½	37	39½	40	Ditto.	Dull miſling day.
25	29,3¼	29,3	29,3	38	35	30	W. little.	Fine pleaſant day.
26	29,6	29,6	29,6¼	23	26	21½	N. W.	Ditto.
27	29,7	29,5½	29,5	9	12	10½	N.	Cloudy dull weather.
28	29,2	29,2	29,1½	7	11	17	Ditto.	Ditto, with rain.
29	29,3	29,3	29,3½	16½	23	26	W. little.	Fair and clear.
30	29,0	28,7½	28,6	20	32	32	N. E. freſh.	Cloudy day; ſtorm of ſnow in the night.
31	28,7	28,9	28,9	26	37	34	N. E. to S.	Cloudy dull weather.

Quantity of rain in January, 2,00 } Inches,
Quantity of water evaporated, 1,55 }

FEBRUARY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,0½	29,1	29,1½	31	37	35	S.	Fair A. M; cloudy P. M.
2	29,3½	29,4	29,6⅓	29	35	32	W. little.	Fine bright day.
3	29,2	29,1¼	29,0	20	23	26½	E. freſh.	Clo. day; hea. ſtor. of ſn. & hail in the nig.
4	28,9	29,2½	29,4½	26	32	31½	S. to N. W. freſh.	Dull cloudy day.
5	29,7½	29,7½	29,9¼	21½	23½	21½	N. W. freſh.	Flying clouds, with bright intervals.
6	30,1	30,1¼	30,1¼	14½	20½	14	N. W. little.	Fine bright day. *Aur. Bor.* in the night.
7	30,0⅙	29,9⅓	30,0¼	23	31	32½	S. W. to W.	Fine pleaſant weather.
8	30,2¼	30,2½	30,0	23	25	31½	N. to E. S. E.	Cloudy dull day.
9	29,4¼	29,1	29,2⅔	37	40	38	S. to W. freſh.	Rainy A. M; cloudy P. M. bright even.
10	29,7½	29,9	30,0	22	25½	26	N. W. freſh.	Fair and clear.
11	30,0	29,6½	29,7	25	30	34½	S. W. to W. freſh.	Ditto.
12	30,2⅔	30,2	30,3¼	20	19½	14	N. W.	Fair day.
13	30,4	30,4	30,4	3	11	11	N. W. to N. E.	Fair day; cloudy evening.
14	30,2	29,9	29,9½	11½	12½	15	N. to W.	Snow A. M; clear P. M.
15	30,1½	30,1	29,2	7½	14	32	W. to E.	Clo. A. M; ſn. with ra. P. M. & in the nig.
16	29,2¼	29,1	29,1¼	27	32	29½	S. W. little.	Fair pleaſant day.
17	29,6	29,6	29,6	16½	26	24	W. to S. W. freſh.	Flying clouds, with bright intervals.
18	29,6	29,6	29,6	25	34	39	S. W. little.	Fine pleaſant day.
19	29,6½	29,6½	29,6½	36½	39	41½	S. W.	Ditto.
20	29,6½	29,6½	29,6½	40½	44½	49½	Ditto.	Cloudy day; rain in the night.
21	29,2¾	29,4	29,6	49½	51	48	W. S. W.	Fine pleaſant day.
22	29,5⅔	29,2½	29,5	44	47½	49	Ditto. freſh.	Ditto.
23	29,6	29,6	29,8	42	43	42½	W.	Ditto.
24	29,8½	29,8½	29,8½	38	40	39½	S. W. to E.	Ditto.
25	29,3½	29,0	28,4½	35	34	35	N. E. to S. ſtrong.	Heavy ſtorm of ſnow and rain all day.
26	28,6	28,6½	29,1	31	32	28½	W. to N. ſtrong.	Flying clouds, with bright intervals.
27	29 3½	29,3½	29 3½	25	33	35	W.	Fine pleaſant day. *Aur. Bor.* in the nig.
28	29,3½	29,4	29,2½	31	37	40	S. W. little.	Ditto.
29	29,2	29,2	29,3½	37	39	32	Ditto. freſh.	Ditto.

Quantity of rain in February, 2,40 } Inches.
Quantity of water evaporated, 1,65 }

MARCH, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,0½	29,5	29,4½	28	31	30	N. W. to S.	Fair weather.
2	29,0	29,3½	29,4	29	36½	31	S. W. little.	Fine bright day.
3	29,0	29,9	29,9½	27	31	33	N. E.	Cloudy dull weather.
4	30,0½	30,0	30,0	30	34	32	N. to W. little.	Fine pleaſant day.
5	29,8	29,4	28,8	27½	33	33	N. E. ſtrong.	Heavy ſtorm of ſnow.
6	28,8	29,1	29,4	31	34	27	W. freſh.	Clear pleaſant weather.
7	29,8	29,8	29,8	16	25	25	W. little.	Ditto.
8	29,9	29,9	29,9½	23	37	34	Ditto.	Ditto.
9	30,2	30,1½	30,0½	26	23½	20½	N. freſh.	Cloudy with ſome ſnow.
10	30,0	30,0	30,0	17½	31	25	E. little.	Fine pleaſant day.
11	29,7½	29,5	28,8	17	27½	31	N. N. E.	Clo. A. M; heavy ſtorm of ſnow P. M.
12	28,7	29,2½	29,8	30	32	25	W. freſh.	Cloudy morning; clear bright day.
13	29,8	29,8	29,8	16	28	26	N.	Cloudy with ſome ſnow.
14	29,8	29,8	30,0½	22	27	20	W.	Clear bright day.
15	30,3	30,2	30,2	12	23	20	Ditto.	Ditto.
16	30,2	30,1	29,6½	10½	25	29½	W. to S. freſh.	Dull cloudy weather.
17	29,6	29,7	29,8½	28	31	28	W. to S. freſh.	Clear bright day.
18	30,0	30,0	29,6½	20	35	36½	W. to S.	Clear A. M; cloudy P. M.
19	29,6	29,4	29,4½	33	49½	39½	S. W. to S. E.	Ditto Ditto.
20	29,4½	29 2½	29,1	34	32½	31½	N. E. to N.	Clo. mor. ſtor. of ſn. P. M. & in the even.
21	28,9	28,9	29,0¾	33	38	38	W. S. W. freſh.	Cloudy weather.
22	29,3	29,3½	29,5½	37	40	42	Ditto.	Flying clouds, with bright intervals.
23	29,6	29,6½	29,6	40½	47	46	S. little.	Fine pleaſant day.
24	29,6	29,6	29,6	39	45	42	S. to S. E.	Cloudy weather.
25	29,6	29,6	29,6	39	46½	42½	S. to E. little.	Fine pleaſant day.
26	29,6	29,6	29,6	36	39	39½	N. to S. E. little.	Dull miſling weather.
27	29,6	29,6	29,6	38	47½	42	S. to E. little.	Fine pleaſant weather.
28	29,7½	29,7½	29,7½	36	40	37½	E. to N. E. freſh.	Cloudy day; rainy evening and night.
29	29,3	29,2½	29,2	36	39	36	E. freſh	Heavy ſtorm of ſnow & rain, all day.
30	29,3	29,4	29,5½	36	36½	35½	N. N. E.	Rainy weather.
31	29,8	29,9	29,8½	36	44	39½	N. to E. little.	Fine pleaſant day.

Quantity of rain in March, 4,95
Quantity of water evaporated, 1,45 } Inches.

APRIL, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,8	29,8	29,8	35	46½	47	S. little.	Fine pleafant weather.
2	29,7¾	29,7½	29,6	37	38½	35	N. E. freſh.	Cloudy dull day.
3	29,3½	29,3½	29,4	32½	35	36	N. ſtrong.	Ditto, with ſome ſnow.
4	29,6	29,6	29,7	33	39½	39½	N. to W. little.	Cloudy A. M.—fair and pleaſant P. M.
5	29,8	29,8	29,8	33½	44	45	W. to S. W. little.	Fine pleaſant weather.
6	29,5	29,4	29,4	41½	50½	45	E. little.	Ditto.
7	29,5½	29,5½	29,5½	39	54	48	S. E. little.	Ditto.
8	29,5½	29,4	29,4	41½	50	48	E.	Clear A. M. clou. P. M. and in the night.
9	29,2½	29,0	29,1	42	39½	38	N.E. to N. ſtrong.	Storm of rain all day.
10	29,2½	29,2½	29,2½	40	50	51	N. W. to S. W.	Fine pleaſant day. [night.
11	29,2½	29,2½	29,1½	46	57	54½	S. W. to S. little.	Fair day; clo. evening; ſmall ſhowers in the
12	29,0	29,0	29,1½	47	48	46	S. W. to W. freſh.	Fair weather. *Aur. Bor.* in the night.
13	29,2	29,2	29 2	41	46	41½	Variable.	Cloudy dull day.
14	29,2	29,2	29,3½	40	48½	47½	S. W. freſh.	Flying clouds, with bright intervals.
15	29,6	29,6	29,8½	41½	49	48	W. by S.	Fine bright day; rain in the night.
16	29,8	29,5	29,5½	44	47	53	S. to S. E.	Rainy all day.
17	29,4	29,5½	29,8	48½	52	50½	S. E. to W. little.	Fair pleaſant weather.
18	29,9½	30,0	30,0½	43	53	49	W. little.	Ditto.
19	30,1	30,1	30,0½	42	48	44	N. E.	Fair A. M. cloudy P. M.
20	29,8	29,1½	29,2	36	38	39½	NE to N W ſtrong.	Storm of rain 'till 4 P. M. clou. evening.
21	29,4½	29,5	29,6¾	38½	44	47	N. W. freſh.	Fair A. M. ſhowers P. M. & in the even.
22	29,7	29,7	29,4½	43½	53½	55	S. W.	Fine pleaſant day.
23	29,3	29,1	29,1½	51	58	53	S. to W. freſh.	Fair A. M. ſhowers P. M. and in the even.
24	29,2	29,2	29,4	45	49	47	Variable.	Dull cloudy day.
25	29,5	29,5	29,6	41	48½	45½	N. E. to W.	Ditto.
26	29,6½	29,6	29,6	39½	55	53	W. to S. S. E.	Fine pleaſant day.
27	29,7½	29,7½	29,5¼	50	64	64½	S. little.	Ditto.
28	29,6	29,6	29,6	57½	59½	62	Ditto.	Ditto.
29	29,8	29,8½	29,3	54	60	58½	E. freſh.	Ditto.
30	29,8	29,6	29,5½	53	65½	69	S. W.	Ditto.

Quantity of rain in April, 2,90 } Inches.
Quantity of water evaporated, 3,45 }

MAY,

MAY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,6	29,5½	29,3	64	75½	77½	S. W.	Fair weather.
2	29,3	29,3	29,3	71	77	75	Ditto freſh.	Do. Lightg. in the night, with thund.
3	29,8	29,9	30,0	58½	55	54	N. W.	Cloudy, with bright intervals.
4	30,2	30,2	30,1⅔	42½	57	59	E.	Clear A.M. clou. P.M. & in the evening.
5	30,1½	30,1½	29,9½	46	48	51	S. E.	Dull cloudy weather.
6	29,9½	29,8½	29,9½	49	55	50½	Variable.	Fine pleaſant day.
7	30,0	30,0	30,0	47½	58½	55	E.	Ditto.
8	30,0	29,9	29,5	49	51	52	N. E. to N. N. W.	Cloudy A.M. rainy P.M. & in the night.
9	29,1	29,1	29,2	51	57	55	S. W. freſh.	Fair A. M. ſmall ſhowers P. M.
10	29,4½	29,4½	29,7	49	52½	49½	N. W. freſh.	Fair and pleaſant.
11	29,8⅔	29,8⅔	29,8½	43	52½	53	W.	Ditto.
12	29,8	29,7	29,6½	50	59½	62	Variable.	Cloudy A. M. rainy P. M.
13	29,7½	29,7½	29,8	52	58	56	W.	Clear and pleaſant.
14	29,8½	29,8	29,7⅓	49	60½	56	Ditto.	Ditto.
15	29,6⅔	29,5⅔	29,4	52½	63	63	S. W. little.	Ditto.
16	29,3½	29,3½	29,3½	52½	67	64	Ditto.	Ditto.
17	29,3	29,3	29,3	52	60	56	Variable.	Ditto.
18	29,5	29,5	29,4¾	54	59	53½	E.	Cloudy A. M. rainy P. M.
19	29,4⅔	29,5	29,5	53	60	57	N. E.	Fair day ; cloudy evening.
20	29,3½	29,3½	29,4½	54	59½	57	S. W. to N. W.	Small ſhowers A. M. cloudy P. M.
21	29,5	29,6½	29,6½	49½	59½	59½	N. W.	Fine pleaſant day.
22	29,5	29,5	29,6	56	70½	65	S. W. freſh.	Ditto.
23	29,7	29,5	29,4	57	72	71	Ditto.	Ditto.
24	29,3½	29,2	29,3½	65	77½	64	S. W. to N. E.	Fair till 3 P. M. cloudy evening & night.
25	29,3½	29,5	29,8	53	57	56	N. to N. E.	Dull cloudy day.
26							Ditto.	Ditto.
27							Ditto.	Ditto.
28							N. E. to S. W.	Ditto.
29	29,7	29,7	29,7½	55	59	56½	S. W. to N.	Ditto.
30	29,7	29,6½	29,6½	55	61	66	N. W. to S.	Fair and pleaſant.
31	29,5½	29,4	29,3	63	70	75	S. W. freſh.	Ditto.

Small ſhowers.

Quantity of rain in May, 1,00 } Inches.
Quantity of water evaporated, 6,35 }

JUNE.

JUNE, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,4	29,4	29,4	65	77	70½	S. W. freſh.	Fair day; thun. & lighten. in the night.
2	29,5	29,5½	29,6½	64	60	60	N. E.	Dull cloudy weather.
3	29,7½	29,8	29,9¼	55	59½	54	Ditto.	Fine pleaſant day.
4	30,0	30,1	30,0½	51	64	61	N. W.	Ditto.
5	30,1	30,1	30,0⅞	55¼	73	67	W. to S. S. W.	Ditto.
6	30,0½	29,7¾	29,6½	61	78	72½	S. W.	Ditto.
7	29,6½	29,5	29,6	63½	72½	65	S. W. to N. N. E.	Ditto.
8	29,5½	29,5½	29,6	62	71½	64	S. S. W.	Ditto.
9	29,8¼	29,8	29,8½	55½	62	54	W.	Ditto.
10	29,8½	29,8½	29,8½	55	62	57	W. to N. E.	Fair A. M; cloudy P. M.
11	29,7⅓	29,7	29,6	55	58	58	N. E. to S. S. E.	Cloudy day; ſhowers in the night.
12	29,3	29,4½	29,6	57	64	62	W. freſh.	Fine bright day.
13	29,6	29,6½	29,6½	54½	67	67	Ditto.	Ditto.
14	29,6	29,6	29,6	65	77	77	S. W.	Ditto. lightening in the evening.
15	29,6	29,5	29,4	73	83	75½	S. W. freſh.	Ditto.
16	29,3½	29,3½	29,5	69½	73	69	S. W. to W. freſh.	Cloudy A. M; rainy P. M.
17	29,5	29,5	29,5	62	66	64	N.	Cloudy dull day.
18	29,5	29,4½	29,5	60	61½	60½	Ditto.	Rainy day.
19	29,4⅔	29,4	29,4	58½	63½	60½	Ditto.	Ditto.
20	29,3½	29,4	29,3½	57	62	64	Ditto.	Ditto. Fair evening.
21	29,4	29,3½	29,4½	59	67½	60	N. to E.	Cloudy dull day.
22	29,4½	29,4½	29,3½	56	56½	54	N. E.	Cloudy, with ſhowers.
23	29,3½	29,4	29,4	53	61	65	N. to W.	Flying clouds with bright intervals.
24	29,4	29,4	29,4	64	70	69	Ditto.	Ditto.
25	29,4½	29,4⅔	29,4	66½	74	71	Ditto.	Ditto.
26	29,3⅔	29,3⅔	29,3⅔	62½	61½	59	N. W. to E.	Cloudy, with rain.
27	29,4	29,4½	29,6	63	71	67	N. W. freſh.	Flying clouds, with bright intervals.
28	29,7½	29,8	29,8½	70	76	73	W.	Fine pleaſant weather.
29	29,8½	29,8	29,7	66½	82	76	S. W.	Ditto.
30	29,8	29,7	29,6	70	83½	79	Ditto.	Ditto.

Quantity of rain in June, 1,25 } Inches.
Quantity of water evaporated, 5,45 }

JULY,

JULY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,4	29,4	29,4	74	78	77½	S. W.	Small ſhowers A. M. fair & pleaſ. P. M.
2	29,4	29,4	29,4	71	77	75	Ditto.	Cloudy, with ſmall ſhowers.
3	29,4½	29,4½	29,3	74	77	73	N. to E.	Ditto, lightening in the evening.
4	29,3½	29,3½	29,2½	71	78½	79	S. W. to W.	Ditto.
5	29,6	29,6½	29,6	73½	80½	75	W.	Fine pleaſ. day. *Aur. Bor.* in the evening.
6	29,6	29,6	29,6	71½	81	73	S. W.	Fair A. M; cloudy P. M.
7	29,8	29,7½	29,5½	72	79	80	S. W. ſtrong.	Fair till 2 P. M. clou. with th. & ra. nig.
8	29,5¼	29,2	29,4½	77	86	76	S. W. to N. W.	Fair day. *Aur. Bor.* in the night.
9	29,6	29,6	29,6	77	81	72	W.	Fine pleaſant day.
10	29,6	29,6	30,0	66	77	67	Ditto.	Fair A. M. ſhower of rain and hail P. M.
11	30,1	30,1	30,1	62	74	68½	Ditto.	Fine pleaſant day.
12	30,0½	30,0	30,0	78	78	74	Ditto.	Ditto.
13	29,9	29,9½	29,9½	70	75	73	W. to S. little.	Ditto.
14	29,9	29,9	29,9¼	70	78	77½	S. W.	Ditto.
15	29,6	29,6½	29,6½	73	77½	73	Ditto.	Fair A. M. a ſmall ſhower P. M.
16	29,6	29,6½	29,6	71	80	76	Ditto.	Cloudy, dull weather.
17	29,6½	29,6½	29,6	74	78	70	Ditto.	Cloudy, with bright intervals.
18	29,8	29,9	29,9	65	77½	72	Ditto.	Fine pleaſant weather.
19	29,9½	30,0	29,9½	72	81	71	Ditto.	Fair A. M. cloudy P. M.
20	30,1½	30,1½	30,1	72	79	70	Ditto.	Fine pleaſant day.
21	30,1	30,0½	29,9	68	75	70	Ditto.	Fair A.M. cloudy P.M. rain in the night.
22	30,0½	29,7	29,7	66	73	70	Ditto.	Cloudy A. M. clear P. M.
23	29 5	29,5	29,6	65	75	71	N. E. to W.	Ditto, ditto.
24	29,6	29,6	29,6½	68	79	72	Variable.	Cloudy, with thunder and rain.
25	29,8½	29,9	30,0	73	82½	76	W. to N.	Pleaſant day. Rem. *Aur. Bor.* in the nig.
26	30,0½	30,0	29,7	74	84½	80½	S. W.	Fair pleaſant weather.
27	29,8½	29,8	29,6½	76	86½	79	S. to S. W.	Ditto. Lightening in the evening.
28	29,8	29,6	29,4	78	95¾	87	Ditto.	Ditto.
29	29,5	29,3	29,3	81½	*96½	81	Ditto.	Ditto.
30	29,4	29,3	29,3	78	85	78	Ditto.	Fair A. M. thunder and rain P. M.
31	29,8	28,9	28,7	72½	72	71½	Variable.	Dull rainy day.

Quantity of rain in July, 2,95 } Inches.
Quantity of water evaporated, 4,60 }

* This obſervation was taken at 1½ P. M. The thermometer being hung in a ſhade in the open air.

AUGUST, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,5	29,5	29,6	65	70	64½	N. W.	Flying clouds, with bright intervals.
2	29,9½	30,0½	30,1	66	73½	69½	W.	Fine pleaſant weather.
3	30,3¼	30,3½	30,3	65	79	69	Ditto, little.	Ditto.
4	30,2	30,2	30,2	64	78	71½	S.	Ditto.
5	30,1¼	29,6	29,2½	67	70	70	N.	Heavy ſhowers; clears up in the night.
6	29,7	29,7	29,6½	71	82	77	S. W.	Fair weather.
7	29,7	29,7	29,7	75	83	77½	Ditto.	Ditto.
8	29,7	29,5	29,3	76	86	80	Ditto.	Fair day; terr. thun. & lightg. in the nig.
9	29,4	29,4	29,5	78	75	70	Variable.	Cloudy A. M; fair P. M.
10	29,6	29,6	29,6	65	67	66	N. E. little.	Cloudy day; ſmall ſhowers in the even.
11	29,5	29,2	28,6½	67	81	75	Variable.	Unſettled weather.
12	28,9	28,9	28,9½	62	79	74	S. W.	Fair and pleaſant.
13	29,2	29,2	29,4½	70	80	80	Ditto.	Ditto.
14	29,5	29,5	29,5	74	80½	76	Ditto.	Ditto.
15	29,5	29,4	29,2½	75	84½	78½	S.	Fair A. M. ſhowers P. M.
16	29,4½	29 4	29,5	70	72	68	N. E.	Dull cloudy day.
17	29,5	29,5	29,5½	65	66	63½	Ditto.	Cloudy day; rain in the night.
18	29,5	29,4	29,6½	60½	60½	59	Ditto.	Dull rainy weather.
19	29,6	29,6	29,7½	59½	68	64½	S. to N. E.	Unſettled weather.
20	29,8	29,5½	29,6¼	62½	63½	65½	Variable.	Ditto.
21	29,9	29,9½	30,0	63	70	67½	W.	Fine pleaſant weather.
22	29,9	29,8	29,6⅓	62½	76	71	S. W.	Ditto.
23	29,5	29,3½	29,2	67	80	74	Ditto.	Fair A. M. ſmall ſhowers P. M.
24	29,8½	29,9½	30,0½	66	67½	62½	N. E. to E. little.	Fine pleaſant day.
25	29,9¼	29,9	28,4½	58	69	67	E.	Cloudy day; heavy ſhowers in the night.
26	28,3½	28,2⅔	28,6¼	65	72	75½	S. ſtrong.	Flying clouds, with bright intervals.
27	29,2½	29,2	29,2½	72	78	75	Ditto, little.	Fine pleaſant day.
28	29,6½	29,7	28,8¼	73	78½	71	W.	Ditto.
29	29,9½	30,0	30,0½	62	72	69	Ditto.	Ditto.
30	30,3	30,3	30,2½	62	74	67½	Ditto.	Ditto.
31	30,1	29,9	29,6½	64	76	76	S. W. little.	Ditto.

Quantity of rain in Auguſt, 3,15 } Inches.
Quantity of water evaporated, 4,35 }

SEP-

SEPTEMBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,6	29,6½	29,7	71	75	73	E. little.	Fair pleaſant weather.
2	29,7	29,9	29,8½	67	69	67	N. to N. E.	Cloudy day; ſtorm of rain in the night.
3	29,2½	29,2	29,2	67½	68	70	N. freſh.	Rainy till 4 P. M. clears up in the night.
4	29,7½	29,8	30,0½	68½	81½	75	W.	Fine pleaſant day.
5	30,1	30,1	30,1	67	70	62½	Ditto.	Ditto.
6	30,1	30,1	30,1	53	61	62½	Ditto.	Ditto.
7	30,1	30,0	29,8½	57	67½	65½	W. to S.	Fair A. M. cloudy P. M.
8	29,8	29,8	29,8	62	64	67	S. to W.	Cloudy day; fair evening.
9	29,6	29,6	29,7¼	65	73	75	S.	Cloudy dull weather.
10	29,4	29,3½	29,4	58½	57½	55	N.	Rainy day; fair evening.
11	29,2	29,1½	29,1	52	56	53	N. ſtrong.	Storm of rain all day.
12	29,1	29,1½	29,2	50	51	52	N. to W.	Rainy A. M. cloudy P. M. fair evening.
13	29,8½	29,9	30,0	51	58	59	W. little.	Fine pleaſant day.
14	30,0	30,0	30,0½	52½	68	63	W. to S. W. little.	Ditto.
15	30,0	29,9	29,9	58	60½	56	W.	Ditto.
16	29,9	29,8	29,8	51	57	55	W. to N.	Fair A. M. cloudy P. M. rain in the night.
17	29,4½	29,3	29,3½	55	57	61	S. to W.	Rainy day; fair evening.
18	29,5	29,5	29,7½	58	65	62	W. little.	Fair day.
19	29,8	29,8	29,8½	54	66½	65½	Ditto.	Ditto.
20	29,8	29,7⅔	29,7	59	64	66½	S. little.	Ditto.
21	29,8	29,8	29,8	62½	63	63	Ditto.	Ditto.
22	29,8	29,8½	29,9	55	62½	59	S. to E.	Ditto.
23	29,4½	29,4	29,4	62	64½	63	S.	Rainy day.
24	29,6⅔	29,6⅔	29,6⅔	59½	58	57	N.	Cloudy dull weather.
25	29,8	29,8	29,8	57	61	55	Ditto.	Flying clouds, with bright intervals.
26	29,8	29,8½	29,9	49	52	55	Ditto.	Fair weather.
27	29,9	29,9½	29,9½	52	59	62	N. to E.	Ditto.
28	29,9	29,8	29,8	59	65	65	S. to S. W.	Fine bright day.
29	29,7	29,6½	29,7	59	60	57	E.	Dull miſling day.
30	29,7	29,7	29,7	57	59	56	Ditto.	Ditto.

Quantity of rain in September, 2,95 } Inches.
Quantity of water evaporated, 4,50 }

OCTOBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,7	29,7	29,7⅓	56	65	58½	N.	Dull cloudy day.
2	29,7⅓	29,6½	29,6½	56½	64	63	W.	Fine plea. day. *Aur. Bor.* in the night.
3	29,8½	29,8½	29,9	52	55	51	Ditto.	Ditto.
4	29,9½	30,0	30,0	43	54	53	Ditto.	Ditto.
5	30,0	29,9½	29,9	47½	56	57	S. little.	Fair weather.
6	29,9⅓	29,9	29,8½	53	58	59	N. E.	Dull cloudy day.
7	29,7½	29,6½	29,5½	57½	56½	55	N.	Storm of rain all day.
8	28,8	28,6	28,5½	53	54	54½	Ditto.	Ditto.
9	28,6	28,8	28,9½	54	56	57	Ditto.	Dull misling day.
10	29,2	29,2	29,6½	54½	57	57½	Ditto.	Ditto.
11	29,6⅓	29,6⅓	29,7½	54	57	60	W. little.	Fair pleasant weather.
12	29,8½	29,8½	29,9½	54	62	60	S. W. little.	Ditto.
13	30,0	30,0	30,0	53½	57	53	W. N. W.	Ditto.
14	30,0½	30,0	30,1	46	55	55	Ditto.	Ditto.
15	30,0½	30,0	30,1	50	55½	59	Ditto.	Ditto.
16	30,2	30,2	30,2½	54	62	59	E. to N. W.	Ditto.
17	30,0½	30,0	29,9	55	62	60	W. to S. E.	Ditto.
18	29,7½	29,7½	29,7½	56½	57½	59	Variable.	Cloudy dull day,
19	29,7	29,6½	29,5	60	65	61½	S. W. little.	Fine pleasant day.
20	29,2	29,2	29,2	57	61	59	N. E. little.	Cloudy A.M. misling P.M. & in the nig.
21	29,3	29,2½	29,2	56½	56	52	N.	Cloudy A. M; rainy P. M.
22	29,2	29,2	29,2	48½	50	48	W. fresh.	Fair weather.
23	29,4	29,4	29,4	42½	50	49	W.	Fine pleasant day.
24	29,4½	29,4	29,3½	42	54	53½	W. to S. W.	Ditto.
25	29,4	29,3	29,3	53	60½	58½	S. W. fresh.	Cloudy A. M; fair P. M.
26	29,3	29,3	29,5½	52½	50½	57½	S. W.	Fine pleasant day.
27	29,6	29,4½	29,2	50	50	48	N. E. fresh.	Clo. day: sto. of ra. P. M. & in the nig.
28	29,2	29,2	29,2	49	49	50	N. E.	Cloudy with some rain.
29	29,2½	29,2	29,2½	48½	53	52	S. W.	Dull cloudy weather.
30	29,1	29,1	29,1½	47½	51	52	S. E. fresh.	Cloudy A. M; rain P. M. & in the nig.
31	29,4	29,5½	29,6	50	52½	52	N. W. fresh.	Flying clouds with bright intervals.

Quantity of rain in October, 6,70 } Inches.
Quantity of water evaporated, 3,75 }

NOVEMBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,6	29,6	29,7	48	53	53	S. W. little.	Fine pleaſant day.
2	29,7½	29,7	29,6½	47½	55	53	S. E.	Fair A. M; clo. P. M. ſho. in the nig.
3	29,7	29,7	29,7	50	51	49	N. W. freſh.	Fair and pleaſant.
4	29,6	29,6	29,5½	42	52	46	S.	Ditto.
5	29,6	29,6	29,6	42	42	36	N. W. freſh.	Ditto.
6	29,9	29,9½	30,0½	35	39	41½	Ditto.	Ditto.
7	30,0½	30,0⅔	30,0⅔	37½	46	45	S. little.	Ditto.
8	30,1⅓	30,3	30,3½	40	48	47	W. little.	Ditto.
9	30,4⅔	30,5	30,4¾	40	47	53	S. E. little.	Ditto.
10	30,2⅓	30,0	29,8	45	49	53	S. freſh.	Cloudy dull weather.
11	29,8½	29,8⅔	30,0¼	47	48	46	N. W. freſh.	Fair weather.
12	30,0¼	30,0⅓	30,0	38	42	42	Ditto.	Fine pleaſant day.
13	29,9	29,6⅔	29,1	35	38	49	S. E. to E. freſh.	Rainy day.
14	29,5⅔	29,6½	29,8	41½	43	41	N. W.	Fair pleaſant weather.
15	29,9½	30,0	30,1½	36½	39	38	S. W. to W.	Ditto.
16	30,3	30,4	30,5	31	35	32	W.	Ditto.
17	30,5¼	30,5⅔	30,5	27	32	32	Ditto.	Ditto.
18	30,3½	30,2⅓	30,3¼	28	34	37½	S. W. to W.	Ditto.
19	30,4	30,4	30,4	33½	40	39	Ditto.	Ditto.
20	30,3¾	30,3½	30,3½	33	43	40	W.	Ditto.
21	30,4⅓	30,4⅔	30,4⅔	37	40	43	N.	Dull cloudy day.
22	30,3⅔	30,3½	30,3	42	43	45	Variable.	Ditto.
23	30,3	30,4	30,4	42	44	42	W.	Fair pleaſant weather.
24	30,4	30,4	30,3	37	38	40½	Variable.	Cloudy dull day.
25	30,2½	30,0	29,9	43	49	48½	S. E.	Dull rainy weather.
26	29,8½	29,7½	29,6	48	51	46	S. E. to E.	Ditto.
27	29,4⅓	29,4⅔	29,6½	43	45	44	N. E. to N. W.	Cloudy day; fair evening.
28	29,7½	29,8½	30,0	38	41	41	W. little.	Fine pleaſant day.
29	30,0½	30,0	29,7½	42	43	50	E. freſh	Cloudy with rain; ſtormy night.
30	29,7	29,6	29,6	50	50½	48	Variable.	Cloudy day; fair evening.

Quantity of rain in November, 3,90
Quantity of water evaporated, 2,45 } Inches.

T DECEM-

DECEMBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,8	29,8 2/3	29,8 1/2	43	45	45	Variable.	Fair day; rainy evening.
2	29,8	29,8	29,7	47	47 1/2	47	S. E.	Rainy weather.
3	29,6	29,6	29,7 1/4	43	43	40 1/2	N. W. freſh.	Cloudy A. M; fair P. M.
4	29,8	29,8 1/3	29,8 1/2	38	39 1/2	35	W. little.	Fine pleaſant day.
5	29,9	29,9	29,9	34	36	36	Ditto.	Ditto.
6	30,0	30,0	30,1	29 1/2	34	32	N. W. to N. E.	Cloudy unpleaſant weather.
7	30,1	30,0 1/2	30,0	25	28	26 1/2	N. W.	Fair day.
8	29,9	29,9	29,9	26	31	30	Ditto.	Flying clouds, with bright intervals.
9	29,9 1/2	29,9	29,8 1/4	29	32	32	W. to S.	Fair A. M. clo. with ſmall ſhowers P. M.
10	29,8	29,8	29,9	32	35	33	S. freſh.	Fine pleaſant day.
11	29,9	29,9 1/2	30,0	32	30	25	N. W. freſh.	Fair and clear.
12	30,0 1/2	30,1 1/4	30,1 3/4	25	30	28	N. W. little.	Ditto.
13	29,9	29,7 1/2	29,7 1/4	29	33	39	S. freſh.	Cloudy day; fair evening.
14	29,8 1/2	29,8 1/2	30,0 1/2	36	39 1/2	39	W. little.	Fine pleaſ. day. *Aur. Bor.* in the night.
15	30,3	30,3 1/4	30,4 1/4	34 1/2	36	36	E.	Fair A. M. cloudy P. M.
16	30,4	30,2 1/2	30,0	33 1/2	36 1/2	41	N. E. freſh.	Cloudy A. M; rainy P. M.
17	30,0	30,0 1/2	30,1	38	40	40	S. W.	Cloudy unſettled weather.
18	30,2	30,2 1/2	30,2 1/2	39	38 1/2	40 1/2	Ditto.	Ditto.
19	30,1 2/3	30,2	30,2 1/2	39	40 1/2	41 1/2	Ditto.	Fine pleaſant day.
20	30,2 1/2	30,2	29,9	25 1/2	38 1/2	41 1/2	N. E.	Cloudy A. M; rainy P. M.
21	29,8 1/2	29 9	30,0 1/2	40 1/2	42	41 1/2	W. little.	Fine pleaſant day.
22	30,1	30,0 1/2	29,8 2/3	38	40 1/2	44	S. little.	Ditto.
23	29,8	29,9	29,9 1/2	42	44	43	W.	Ditto.
24	30,0	29,9	29,8 1/2	40	40	38	N.	Cloudy day; ſnow in the evening.
25	29,7 1/2	29,9	30,0 1/4	36	37 1/2	35	W.	Fine pleaſant day.
26	30,1	30,1	29,9 1/2	28	32	38	S. W. to E.	Cloudy day; rain in the night.
27	29,5 1/2	29,4 1/2	29,2	42	42	41	Variable.	Dull miſling day.
28	29,4	29,6	29,7	41	41	39 1/2	N. W.	Fine pleaſant weather.
29	29,8 1/2	29,8 2/3	29,6 1/2	38	43	45	Wby S. little.	Ditto.
30	29,6	29,6 2/3	29,9	43	44	35	N. W.	Ditto.
31	29,9 2/3	29,9	29,9	33 1/2	34	35	S.	Cloudy, dull weather.

Quantity of rain in December, 2,15
Quantity of water evaporated, 3,10
} Inches.

The

The greateſt height of the barometer this year, was on the 17th of November: The mercury was then at 30.5$\frac{2}{3}$ inches. The leaſt height was 28.2$\frac{2}{3}$ inches on Auguſt the 26th. The thermometer on the 29th of July, roſe to 96°$\frac{1}{2}$; on February the 13th, it ſtood at 3°: The former was the greateſt, the latter its leaſt height. The quantity of rain that fell in the year was 36.30 inches. The quantity of evaporation, meaſured by filling the veſſel once a week, came out 42.65 inches.

It is however to be obſerved that different methods of meaſuring the evaporation, will lead to different concluſions. It was becauſe the tube was of too ſmall a diameter, and not filled often enough, that the quantity of evaporation came out ſo ſmall in 1771. The method of making the experiments being altered, the evaporation turns out very different in 1772. If the experiments could be made on the ſurface of a watery fluid the reſult would determine the quantity of evaporation, with much greater certainty and accuracy, than can ever be done by means of a veſſel ſuſpended in the air.

OBSERVATIONS IN 1773.

The quantity of rain which fell in

January, was	1,95
February,	0,95
March,	1,75
April,	1,90
May,	2,10
June,	1,70
July,	1,00
Auguſt,	4,15
September,	1,05
October,	4,10
November,	1,90
December,	4,00

To meaſure the quantity of *rain*, I fixed a tube about three inches diameter, in ſuch a manner as to receive the rain as it fell; which I meaſured as ſoon as it was over, and added up the whole of each month together. In this account the *ſnow* and *hail* are included: Theſe were meaſured by taking up in the tube all the ſnow or hail that fell on a ſpace equal to its ſurface, and then melting it. The method of meaſuring the quantity of *evaporation* by a tube ſuſpended in the air being uncertain and inaccurate, thoſe obſervations are omitted.

The greateſt height of the barometer this year, was on the 22d and 23d of February: The mercury at 12^h on each of theſe days, was at 30.6 inches. The leaſt height was $28.6\frac{1}{2}$ inches, on December 26th at 12^h. The thermometer on the 8th of July, at $12^h\frac{3}{4}$, roſe to $96^\circ\frac{1}{2}$: On February the 22d, at $8\frac{1}{4}$ A. M. it ſtood at $9^\circ\frac{1}{2}$ below 0. At both theſe times the thermometer was hung in the open air, in a ſhade: The former was the greateſt, the latter its leaſt height. The quantity of rain this year amounted to 26,55 inches.

On the 17th of July there was an *Aurora*, uncommon in this reſpect, that there were ſeveral appearances of it in the *ſouth*: The firſt of theſe was about 9^h. It began about 20° above the horizon and inſtantly ſpread itſelf in a horizontal direction to the diſtance of 30° each way from the meridian. For the ſpace of one quarter of an hour there were five ſuch appearances, all in the ſouth as before; their duration was not more than half a minute, and their colour a pale light, exactly like that of the Aurora in the north.

There was alſo a remarkable HURRICANE this year, the effects of which were principally felt at *Salisbury*, *Amesbury*, and *Haverhill*. Theſe towns lie on Merrimack river, on the north ſide; Saliſbury being the place where the river empties itſelf into the ocean.

The

The hurricane came on *Auguſt* 14th, 1773. Its *riſe* was very ſudden, and without any previous uncommon appearance in the ſky, or other ſymptom of its approach. In the morning there was a light breeze of wind at the eaſt, attended with plentiful ſhowers. At $7^h \frac{3}{4}$ the wind veered about to ſouth-eaſt, where it became a briſk gale. In about two minutes, it got into the ſouth-weſt, and became on a ſudden very violent. From thence in about two minutes more, it ſhifted to weſt-north-weſt, and then ſuddenly died away to a moderate breeze. While the wind was thus changing, it ſeemed to blow in every direction; the guſts became very violent, and formed many little whirlwinds all around, attended with a very heavy ſhower of rain, and an uncommon darkneſs.

At the place where *Salisbury* and *Amesbury* join, the *violence* of the hurricane was very great. Its firſt appearance was on Merrimack river. At the time when the wind was veering to the ſouth-eaſt, its waters ſeemed to be inſtantly thrown into a violent agitation; and came rolling from the eaſt as if they would have overflowed the banks. The hurricane immediately ſtruck the ſhore at Saliſbury-Point and Ameſbury, levelling before it ſeveral well built houſes almoſt new, unroofing, twiſting, and wrecking others; and thus tearing down, or ſhattering moſt of the buildings that were in its way. Several buildings were ſhattered to pieces, and others removed in an inſtant. A ſail-maker's loft in which a man was ſitting, was carried away and diſperſed in a moment; the unhappy man being found ſenſeleſs at the diſtance of 94 feet from the place where the loft ſtood. A large oak poſt 14 feet in length and 11 inches in diameter, was taken up and carried by the wind 138 feet. Two new veſſels of 90 tons burthen, were lifted up from the blocks and carried to the diſtance of 22 feet. And a large bundle of ſhingles was taken up from the earth and thrown near 330 feet, in a direction contrary to that of the poſt and veſſels. The trees around

were

were torn up, the fences were thrown down, ſcattered or carried off, and the various kinds of lumber that lay diſperſed on the ſhore, were whirled about in different directions, and to different diſtances. Some houſes and veſſels that ſeemed the moſt expoſed to the wind, ſuffered nothing at all; and others that ſeemed to be the leaſt expoſed, were much damaged or carried off. The number of buildings that ſuffered was about 120: And though many perſons were carried to ſome diſtance, and others much hurt, being covered in the ruins or in the cellars of their houſes, no lives were loſt. At the place where this deſtruction was done, the buildings were pretty thick, amounting to about 150. The general *direction* of the hurricane was from eaſt-ſouth-eaſt to weſt-north-weſt. Its *extent*, in width, was about a quarter of a mile; in length, about a mile and three quarters: And its *duration*, not more than four minutes from the time it firſt began till all became ſtill and quiet again.

At the ſame time conſiderable damage was done at *Haverhill*, ten miles higher up the river. There the hurricane came on from the ſouth-weſt. Its firſt effect was the deſtruction of a large new barn. The barn containing a large quantity of hay and grain, was cruſhed down in an inſtant. The hay, boards and ſhingles were ſcattered round to all points of the compaſs, to the diſtance of four or five rods from the place where the barn ſtood; and ſome of them were carried to the diſtance of three miles north-eaſt. A large dwelling houſe at eight or ten rods diſtance, was much damaged; every board and rib was torn from the roof, and the chimney wrecked to the foundation. Five barns were almoſt wholly deſtroyed, and a number of houſes and other buildings were much damaged. In ſome, the windows and doors were drove in; in others, the boards and ſhingles were ripped off and ſcattered in all directions. The ſtone walls in ſome places were almoſt levelled with the ground; and the trees to the number

number of five hundred were ſwept off in the ſpace of a mile. The general *direction* of the hurricane was different here from what it was in Saliſbury, being from ſouth-weſt to north-eaſt. Its *extent* was about three miles in length, and half a mile in width: Its *duration* not more than four minutes. The *violence* of the hurricane was probably as great at Haverhill, as it was at Saliſbury: But as it paſſed half a mile above or north-weſt of the centre of the town, where the buildings were not very thick, the damage done by it was much leſs.

To what *extent* the diſturbance in the atmoſphere reached, cannot be exactly determined. Though it did not form whirlwinds of ſuch force as to produce any remarkable effects at any other places but thoſe mentioned above, it evidently extended to all the towns on Merrimack river, from the mouth to ſome miles above Haverhill. In all the adjacent towns, the ſudden change and different directions of the wind, with their effects in twiſting the trees, corn, &c. were obſerved to the diſtance of ſix or eight miles on each ſide of the river. And yet there did not ſeem to be any very great alterations in the *weight* or *temperature* of the atmoſphere, at any conſiderable diſtance from the place where the winds were ſo violent. At *Bradford* oppoſite to Haverhill, and not more than a mile from the place where the damage was done, the *barometer* at 7^h A. M. ſtood at 29,8 $\frac{1}{2}$ inches. At the time of the hurricane it ſuddenly fell to 29,6 $\frac{1}{4}$; and juſt after to 29,5 $\frac{3}{4}$, which was its leaſt height that day. By noon it roſe to 29,7 $\frac{1}{2}$, and at 9^h P. M. it got up to 29,8 $\frac{1}{2}$; alterations very common in this part of America. *Farenheit*'s *thermometer* in the morning was at 74°; at noon and 9^h P. M. it was one degree lower: The wind continuing very moderate between weſt and north-weſt from 8^h A. M. till night.

From theſe phenomena we may form ſome probable conjectures as to the *cauſe* and *origin* of the hurricane. What occaſioned ſuch a violent irregular commotion in the air,

air, was probably the great *rarefaction* of it. The weather, for a week before, had been uncommonly hot, and the wind conſtantly at ſouth or ſouth-weſt. The air next to the ſurface of the earth muſt therefore have been greatly heated and rarefied; and probably was become ſpecifically lighter than the air in the higher regions. The conſequence would be that the lighter rarefied air would aſcend, while the heavier condenſed air would deſcend; and in this way the *equilibrium* of the air would be deſtroyed: To the place where the equilibrium of the air was thus broken, the adjacent air would inſtantly flow on every ſide as to a common centre; forming eddies and whirlpools, and thus aſſuming a *circular motion.* And this it is probable gave riſe to thoſe ſudden changes, different directions, and violent guſts of the wind. The place where this circular motion began, ſeems to have been in the upper region of the air, at ſome diſtance from the ſurface of the earth; for ſeveral of the effects of the hurricane bear the marks of a *deſcent.* At the place where it firſt ſtruck Merrimack river, it hove up the waters, as if ſome great force had been impreſſed upon its ſurface: And at Haverhill, where it was firſt felt it cruſhed a barn to pieces as if ſome immenſe weight had fallen upon it. When the deſcending air and whirl came to the earth, being ſtopped in its deſcent it ſeems to have inſtantly ſpread itſelf in to a larger circle or compaſs, blowing every way from the centre. And hence the pieces of the barm that was cruſhed down by it, became ſcattered to the diſtance of four or five rods all around. A deſcent of the air in one place would be immediately ſucceeded with an *aſcent* of it in another. And thus the whirlwind, where the air was deſcending cruſhed down the buildings before it: But where the air was aſcending, lifted up, unroofed, or carried them away; ſhattering and throwing off the materials thus carried up, as they came to the extremity of the whirl, in tangent lines, to different diſtances and in all directions. Beſides this circular, the whirl-

winds

winds had alſo a *progreſſive* motion. Had the different winds by which the whirlwinds were formed been of equal violence, the whirlwinds would have been ſtationary conſiſting only of a circular motion; but being of unequal violence, the whirlwinds had a progreſſive motion, proceeding in different directions at different places, according to the direction of the ſtrongeſt wind.

The *ſummer* preceding this hurricane had been in ſome reſpects different from what is common in this part of the country. There had been an uncommon drought for two months before, which was no where more ſevere than in the towns upon the river; and in no ſummer for ſeveral years, have we had ſo much hot weather. This circumſtance is agreeable to the preſumption of theory; for, if whirlwinds and hurricanes are derived from the great rarefaction of ſome part of the atmoſphere, it might be expected that the times in which they would happen, would be in the moſt calm, or hot weather.

N° X.

A Letter from J. MADISON, *Eſquire, to* D. RITTENHOUSE, *Eſquire.*

William and Mary College, Virginia, November, 1779.

DEAR SIR,

AGREEABLY to promiſe, I now tranſmit you a ſeries of obſervations upon our climate. They comprehend an entire year, and part of the ſucceeding. I thought once of ſending you only a mean of the obſervations for each month, but as it was a part of our natural hiſtory, which has never yet been made public, I have therefore ſent a copy of the journal. Some ſingular cir-

cumſtances too attending the barometer I thought deſerved to be particularly noted, which could not have been done had the firſt idea been adopted. For the obſervations upon the barometer not only ſhew us the different ſtates of the atmoſphere, but, perhaps, may throw farther light upon the true cauſe of the Aurora Borealis. The fact is, that a fall of the barometer always ſucceeds that phenomenon. The frequency of its appearance lately, gave me an opportunity of obſerving this effect at different times. It has for ſome time been ſuppoſed (after Dr. Franklin had firſt given riſe to the opinion) to be an electrical appearance; and I think, the levity of the atmoſphere, as proved by the barometer, adds great weight to that ſuppoſition: ſince it is well known to every electrician, that a rarefaction of the air, in our experiments, will always produce ſimilar appearances. One circumſtance indeed was obſervable, that a change of weather, to wet, generally ſucceeded; but as this effect was not ſo conſtant, it was not much attended to. But the barometer by ſhewing that the atmoſphere is actually lighter, and of conſequence more rarefied at the time of ſuch appearances than at others, evinces at leaſt, that it is in a ſtate the moſt likely to exhibit them; it is to be obſerved alſo, that the greateſt fall of the barometer is not prior to, but always ſucceeds this appearance; ſhewing that the rarefaction firſt begins in the upper parts of the atmoſphere.

It is remarkable that the range of the barometer was not more than one inch and a tenth throughout the whole year, nor do I remember ever to have ſeen a greater difference at any time not included in the journal; whilſt we ſee in other countries, the atmoſphere undergoing changes ſo great as to effect a difference of three or four inches. Whence is it then that we are expoſed to more violent ſtorms of wind and rain? Perhaps indeed the changes here, though not ſo great, may be more ſudden, of which ſome remarkable inſtances may be ſeen in the journal.

Our

Our coldeſt winds, as well as the moſt violent, are the north-weſt. The ſouth and ſouth-weſt winds are the hotteſt, though the ſenſations of heat to which we are expoſed, do not correſpond to the different degrees marked by the thermometer, as they depend much upon a current of air with which we are generally favoured about the hotteſt time of the day, and copious ſweating. I do not recollect ever to have ſeen the thermometer here at more than 95, though Dr. Franklin mentions that in June 1750, it ſtood at 100 in the ſhade at Philadelphia, when he obſerves, "I expected that the natural heat of the body, 96, added "to the heat of the air, 100, ſhould jointly have created "or produced a much greater degree of heat in the body; "but the fact was, that my body never grew ſo hot as the "air that ſurrounded it, or the inanimate bodies immerſ-"ed in the ſame. For I remember well, that the deſk, "when I laid my arm upon it, a chair when I ſat down "in it, all felt exceeding warm to me, as if they had been "warmed before the fire. And I ſuppoſe a dead body "would have acquired the temperature of the air, though "a living one, by continual ſweating, and by the eva-"poration of that ſweat was kept cold." I have been the more particular in tranſcribing this paſſage from the works of this philoſopher, as it certainly ſhews to whom the merit of certain late diſcoveries, which have made ſo much noiſe in the philoſophical world, moſt juſtly belongs; I mean, that power which the human as well as all animate bodies have, of counteracting the heat of an atmoſphere in which they are placed. For what do all the experiments upon heated rooms evince, farther than had before been publiſhed by the doctor? It is thus that Franklin ſetting in his chair, like Newton reaſoning upon the figure of the earth, could ſhew what muſt coſt others infinite labour and fatigue. But, though the effect was obſerved and attributed to evaporation, yet I do not remember that it is any where ſhewn in what manner eva-

poration produces cold. Hamilton, in his excellent eſſay upon the aſcent of vapors, ſpeaking of the natures of ſolution and evaporation, has theſe words, "how cold is "produced in either caſe, I cannot pretend to ſay." The doctor has given the moſt probable explanation of the manner in which it is produced by ſolution, and I think the following, which is collected from his general doctrine may be applied to evaporation. It is admitted that there is a ſtronger attraction between heat and water, or ſuch like fluid, than between heat and any other body, for on this account it is that bodies are cooled when plunged into water. When ever therefore, water for inſtance, is put upon any part of the human body, its natural heat is more attracted in that part by the water, than by the fleſh, and therefore, the water in going off in the form of vapour carries with it part of the heat, and conſequently leaves that place in a negative ſtate, or with leſs than its natural quantity. It is the ſame with the thermometer. Hence it is, that we are much hotter frequently when the thermometer ſcarcely exceeds 82 or 83, there being no current of air to carry off the moiſture from the ſurface of the body, than when it even ſtands as high as 90 or 95.

I am, with the greateſt reſpect,

Your ſervant and friend,

J. MADISON.

JULY,

JULY, 1777.

The Obfervations upon the Thermometer were made at eight, twelve and four o'clock, in the fummer. In the winter, the laft at three, the other obfervations at eight o'clock.

Days.	Winds.	Weather.	Barometer,	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
3	N by E	Clear	30 1 3		73	73½	
4	N b E	Clear	30 1 0	69½	73	74	
5	N b E	Clear	30 0 0	70⅔	74	76,5	
6	N E	Rain	29 7 0	74	77		
7	E b N	Cloudy & rain	30 1 0	73	73	73,5	
8	E b N	Cloudy & rain	30 1 5	72	74	76	
9	S b W	Clear	30 0 8	75	78	79	
10	E b N	Clear	30 0 2	78	83	84	
11	S	Clear	30 0 2	79	82	83	
12	S W	Clear	29 8 4	80	83	85	
13	N b E	Rainy	29 7 2	79	80	81	
14	E b S	Cloudy	29 9 0	75	78	76	
15	E b N	Cloudy	29 9 0	75	78	80	
16	S E	Clear	29 9 0	79	82		
21	S W	Clear	29 9 4	77	81	82	
22	S W	Cloudy	29 8 7	79	82	82	
23	N W	Clear	29 9 2	74	75	75	
24	N b W	Clear	30 0 5	74	75	74	
25	N b E	Cloudy	30 0 5	73	73	74	
26	S W	Clear	29 9 5	74	77		
27	N b E	Clear	29 9 9	77	80	80	
28	S W	Clear	29 9 0	80	82	82	
29	W	Clear	29 3 0	80	81	82	
30	N b W	Clear	30 0 0	78	79	80	
31	N E	Clear	30 1 3	77	78	79	

Note, That the height of the mercury in the barometer is determined by a fcale divided into inches and tenths with a nonius; fo that 30.1.3 will be read 30 inches, 1 tenth, and 3 tenths of a tenth.

AUGUST, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S W	Clear	30 1 3	77	80	83	
2	S W	Clear	30 2 4	79	83	84	
3	S W	Clear	30 1 8	83	85	88	
4	S W	Clear	30 0 8	83	85	86	Much lightening in the evening.
5*	S W	Clear	30 0 5	81	86	87	* Rain with lightening. The barometer was
6	S W	Rainy	30 0 5	82	83	82	obſerved to riſe before the rain to 30 1 0.
7	S W	Cloudy	30 0 9	80	82	80	
8	S W	Clear	30 2 2	80	82		
9	S W	Clear	30 2 2	80	84		
10	S W	Clear	30 1 7			86	It was hotter according to the ſenſation of the
11	S W	Clear	30 1 7	83	86	88	human body this evening than had been obſerved
12	S W	Clear	30 1 5	83	87	88	this ſummer. There was no wind.
13	S W	Clear	30 1 8	84	86½	88	
14	S W	Clear	30 1 3	84	86	89	
15	S W	Clear	30 0 8	84	86	91	
16	S W	Clear	30 1 0	86	88	87	Much rain and lightening. The point was dri-
17	W	Clear	30 1 4	82	84		ven off the Capitol rod, conſiderably fuſed.
18	W	Clear	30 1 4	83	85		
19	E	Cloudy	30 1 4		82		
20	W	Clear	30 1 4	79	82	83	Frequent rain.
21	W	Clear	30 1 4		82	85	
23	S W	Clear	30 1 4	82	85	86	
24	S W	Clear	30 1 2	85	86	83	Rain with lightening.
25	S W	Clear	30 1 2	83	85	83	
26	S W	Clear	30 0 6	80	84		Rain.
27	N W	Cloudy	30 0 0	82	79	79	
28	N E	Cloudy	30 1 5	76	76	76	
29	N E	Clear	30 2 6	73	75	75	
30	N E	Clear	30 1 1	73	74		
31	S W	Clear	30 2 0	74	76		

SEP-

SEPTEMBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S E	Clear	30 1 8	73	76	77	
2	N E	Clear	30 1 8	75	78	78	
3	N E	Cloudy	30 3 0	72	73	72	
4	N E	Cloudy	30 3 3	69	71		
5	S E	Clear	30 2 5	73	75	74	
6	S W	Clear	30 1 9	72	78		
7	S	Clear	30 1 0	78	82	80	Rain.
8	N E	Clear	30 1 7	75	76	75	
9	E	Clear	30 3 0	76	76	75	
10	S W	Cloudy	30 1 7	74	79	79	
11	S E	Clear	30 0 5	79	81		
12	S E	Clear	29 9 4	81	82	80	Rain.
13	N E	Clear	30 1 1	76			
14	S E	Clear	30 2 7	73	73	72	
15	N E	Cloudy	30 2 6	72	74	73	
16	E	Rain	30 1 6	72	71	70	
17	N W	Cloudy	29 9 0	69	69	68	Rain.
18	N E	Cloudy	29 9 0	68	67½	67	Showery.
19	E	Clear	30 0 6	69	70	70	
20	S W	Clear	30 0 6	69	77	77	
21	E	Clear	30 0 0	75	79		
22	N E	Clear	30 0 8	67	69	71	
23	N E	Clear	30 0 8	67	69	70	
24	S W	Clear	30 0 5	69	72		
25	S W	Clear	30 0 0	69	74	71	Rain.
26	N W	Cloudy	30 1 5	60	63	63	
27	N E	Clear	30 2 0	61	63	62	
28	N E	Cloudy	30 1 9	59	60	60	
29	N E	Clear	30 1 6	59	61	61	
30	N E	Cloudy	30 2 6	58	60	60	

OCTO-

OCTOBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	N E	Clear	30 3 0	58	61	62	
2	W	Clear	30 3 0	61	64	65	
3	S W	Clear	30 3 0	62	65	66	
4	S	Clear	30 2 6	67			
5	S	Clear	30 2 7	70	44	75	
6	S E	Clear	30 2 0	70	74	75	
7	N E	Cloudy	30 1 1	72	72	72	
8	N E	Clear	30 2 4	66	68	68	
9	S	Rain	30 0 5	67	72	71	
10	N E	Clear	30 .3 0	64	65	65	
11	N	Cloudy	30 3 3	62	62	62	
12	N E	Cloudy	30 1 9	62	62	63	
13	S W	Cloudy	30 1 5	61	63	64	
14	N E	Clear	30 2 1	62	64	65	
15	N E	Clear	30 2 3	61	61	62	
16	N W	Cloudy	30 0 3	58	60	62	
17	N E	Cloudy	29 9 9	59	61	63	
18	N E	Clear	30 0 4	61			
19	S W	Clear	30 1 7	66			
28	N by E	Rain	29 6 8*	56	57		Much rain. * The loweſt.
29	S W	Cloudy	29 7 0	48			
30	W b S	Clear	30 0 0	49	59	65	
31	W	Clear	30 2 0	50	60		

NOVEM-

NOVEMBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Observations.
1	N E	Clear	30 5 0	56	53	52	
2	N E	Cloudy	30 4 8		58		
10	W	Clear	30 1 3	42	49	50	The ground frozen.
11	S W	Clear	29 7 0	42	52	55	
12	W	Clear	29 9 0	47	55	58	
13	W b N	Clear	30 0 8	41	45	49	
15	N	Cloudy	30 0 8	45	47	49	
16	N	Cloudy	30 0 9	44	45	47	Froſt.
17	N	Clear	30 0 0	41	52	54	Wind high and cold.
18	N	Clear	30 3 7	37	42	45	* Remarkable Aurora Borealis at 7 o'clock
19	N	Clear	30 2 9	33	39	43	this evening. It was terminated towards the
20	N W	Clear	30 2 4	37	48	50	eaſt by the two ſtars β and θ in Auriga,
21	S W	Clear	30 2 4	39	55	57	and its greateſt altitude reached nearly Capel-
22	W b S	Clear	30 1 0	54	59	61	la. The ſtars in the tail of the Great Bear ter-
23	S W	Clear	30 0 7	55	59	62	minated it to the weſt. It is obſervable that
24	S W	Clear	30 0 5	57	59	63	the barometer was falling from the 18th, and
25	W	Clear	30 0 3	55	56	59	was never obſerved *ſo low* but once, little after
26	W	Clear	30 0 2	57	59	60	the vernal equinox, throughout the whole year,
27*.	N W	Clear	30 0 0	36		48	as it was ſo ſoon after the Aurora. Its ſudden
28	E b S	Cloudy	29 8 4	42	42	41	riſe was alſo remarkable. It became cloudy
29†	N W	Snow	29 3 9	41	39	37	about one o'clock.
30	N W	Rain	29 4 4	39	39	40	† Snow fell $7\frac{1}{2}$ inches in 24 hours.

DECEMBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Observations.
1	W	Clear	29 7 7	35	45	47	
2	W	Clear	30 4 0	37	51		
3	W b S	Clear	30 4 0	37	48	50	
4	W b N	Clear	29 9 6	44	53	55	
5	N	Clear	30 1 0	40			
6	N	Clear	30 3 8	41	49	51	
7	S W	Clear	30 2 3	43	58	60	
8	S W	Clear	30 0 4	54	62	67	
9	N	Rain	29 8 0	52	47	46	Snow in the evening.
10	N b W	Cloudy	30 1 0	41	45	47	
11	N W	Clear	30 2 8	36	45	46	
12	S W	Clear	30 1 2	36	47	50	
13	S W	Clear	30 0 0	40	57	60	
14	N W	Cloudy	30 1 2	45	50	57	
15	N W	Clear	30 1 0	46	58	60	
16	W	Cloudy	30 0 0	46	59	62	
17	S W	Clear	29 9 0	49	51	59	
18	N E	Cloudy	29 9 0	47	51	56	
19	N b W	Clear	29 8 9	48	50	56	
20	N	Clear	30 0 8	46	49	51	
21	W	Clear	30 1 0	36	45	49	
22	S W	Clear	30 0 0	40	47	55	An Aurora.
23	N	Clear	30 0 0	43	44	45	Cloudy evening.
24	N E	Snow	30 0 0	40	41	43	
25	N	Cloudy	30 0 4	35	46	48	
26	S W	Cloudy	30 1 0	41	49	52	
27	S W	Cloudy	29 9 0	50	56	59	
28	N E	Rain	29 9 0	41	44	46	
29	N W	Clear	30 1 0	35	37	39	
30	N	Clear	30 4 6	25	27	28	
31	E by N	Clear	30 4 3	27	32	38	

JANUARY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	N E	Clear	30 3 9	30	39	40	
2	S W	Cloudy	30 3 4	40	47	49	
3	S W	Cloudy	30 1 0	50	55	58	
4	W b N	Clear	30 0 0	41	57	59	
5	S W	Clear	30 1 0	50	60	63	
6	S W	Clear	30 2 0	55	62	66	
7	N b W	Clear	30 3 0	50	56		
8	W	Rain	30 1 0	45	52	57	
9	W	Clear	29 6 0	48			
10	S W	Clear	29 9 3	47	58	61	
11	N	Rain	29 9 7	48	47	47	
12	N W	Clear	30 1 3	43	47		
13	N	Clear	30 4 3	37	50	50	
14	W	Clear	30 1 8	47			
15	N	Clear	30 2 9	30	37	39	
16	N	Clear	30 3 5	27	40	43	
18		Clear	29 7 7	41			
19	W	Clear	30 0 1	41	45	47	
20	W	Cloudy	29 9 0	40	43	46	
21	S W	Cloudy	29 8 0	40	51	56	
22	N W	Clear	29 8 1	41	52	55	
23	N E	Cloudy	29 9 3	34	37	40	
24				37			
25	S W	Cloudy	29 8 0	47	50	57	
26	N W	Clear	30 1 0	38	42	44	
27	N W	Clear	30 3 2	32	40	43	
28	S W	Cloudy	30 2 0	38	52	57	
29	S W	Rain	29 9 3	61	62	63	
30	N W	Clear	30 3 2	47	55	56	
31	S W	Rain	30 2 0	48	54		

FEBRU-

FEBRUARY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Observations.
1	S W	Clear	30 3 0	54	59		
2	W	Cloudy	30 4 0	43	48	49	
3	W by S	Clear	30 3 3	40	53	57	
4	S W	Clear	30 3 3	56	60	61	
5	S W	Clear	30 3 0	52	61	62	
6	S E	Clear	30 3 0	57	57	57	
7	N W	Clear	30 1 8	49		55	
8	N E	Clear	30 2 7	45	44	44	
9	N W	Clear	30 2 7	31	40	41	
10	S E	Cloudy	30 1 3	35			Much rain in the evening.
11	W	Rainy	30 1 4	42	43	43	
12	N W	Clear	30 0 7	42	47	47	
13	W b N	Clear	30 0 9	40	47	48	
14	W	Clear	30 1 7	40	45	46	
15	E	Rainy	30 1 6	40	42	43	
16	W	Cloudy	30 1 0	41			
17	N W	Clear	30 3 3	31	40	42	
18	N W	Clear	30 3 4	32	41	42	
19	S W	Cloudy	30 1 0	39			
20	S W	Rain	29 8 3	45	46		
21	N W	Clear	30 1 0	38	45	46	
22	F b S	Cloudy	30 0 5	36	37	37	
23	W	Clear	29 7 7	39	49	47	
24	W b N	Clear	30 0 2	39	54		
25	S W	Cloudy	30 0 2	47	58	60	
26	S W	Cloudy	29 9 0	60	70	70	
27	W	Rain	29 6 6	64	60	59	
28	W b S	Clear	29 9 0	42	51		Wind W at 8.

MARCH,

MARCH, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	W	Clear	29 9 0	36	47	48	
2	N W	Clear	30 0 6	35	41	44	
3	E b N	Cloudy	27 6 0	36	32	31	Snows very faft.
4	N W	Clear	30 0 6	22½	35	39	
5	S W	Clear	30 0 6	23	42	45	
6	W	Clear	30 2 7	42	49	53	
7	S W	Clear	30 2 5	47	57	60	
8	E	Clear	30 2 0	45	46		
9	E	Cloudy	30 1 8	45	48	49	
10	N	Cloudy	30 1 9	45	47	57	
11	S W	Cloudy	30 1 5	47	52	62	
12	S W	Clear	30 0 4	59	69	70	Peach trees in bloffom.
13	S W	Clear	30 0 5	68	71	76	
14	S W	Clear	30 0 6	69	72	79	
15	S W	Cloudy	30 1 7	65	66	65	The wind at N W for part of the day.
16	S W	Cloudy	30 0 8	63	74	76	
17	S W	Clear	29 8 6	67	67	65	
18	N	Clear	30 2 9	51	51	58	
19	S	Cloudy	30 1 9	47	54	55	
20	N W	Clear	29 9 6	50	50	60	
21	N W	Clear	29 9 4	49	51	59	
22	N	Clear	30 2 6	39	47	54	
23	N b E	Cloudy	30 1 0	44	45	56	
24	E	Clear	30 0 6	45	51	53	
25	E	Rain	29 9 6	45	47	48	
26	S E	Rainy	29 6 5	48	57		
27	N	Clear	29 7 1	46			
28	N W	Clear	29 8 0	47	56	57	
29	S E	Rainy	29 3 0	50	54		Winds very high.
30	N W	Cloudy	29 5 0	45	48	49	Ditto.
31	N W	Clear	29 9 0	47	49	54	

W APRIL,

APRIL, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Observations.
1	N	Clear	29 9 0	48	54	55	
2	S E	Clear	29 7 3	54	57	59	
3	W	Clear	29 7 8	45	54	55	
5	W	Clear	29 7 7	44	57		
6	W	Clear	29 8 0	47	59	60	
7	N W	Cloudy	29 8 0	52	56	58	
8	N W	Clear	30 0 0	52	60	61	
9	E	Clear	30 2 0	51	60	61	
10	S W	Clear	30 2 0	51	63	64	
11	S E	Rainy	30 1 8	59	60	61	
12	E b S	Rainy	30 1 0	58	60	60	
13	S	Clear	30 1 3	59	63	70	
14	S W	Rainy	30 1 0	61	73	71	
15	S W	Rainy	30 0 5	66	67	69	
16	N	Rainy	29 9 0	59	62	65	
17	N W	Clear	29 7 7	58			
18							
26	W	Clear	29 8 6	56	65	61	
27	E	Rainy	29 9 3	58	56	56	
28	E b N	Rainy	29 7 3	57	58	57	
29	E b N	Rainy	29 5 7	57	57	57	
30	N b W	Clear	29 6 5	56	59	59	Lightening and thunder.

MAY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	N W	Clear	29 6 8	54	58	59	
2	N E	Clear	29 6 2	55	57	58	
3	N E	Clear	29 7 6	55			
4	N W	Clear	29 6 3	56	62	63	Rain.
5	N W	Clear	29 8 8	61	68	71	
6	S W	Clear	29 8 2	62	72	77	
7	S E	Clear	29 8 0	67	76	79	
8	N	Clear	29 8 6	65	66	69	
9	W	Clear	30 0 4	59	60	66	
10	S	Clear	30 0 3	65			
11	W	Clear	29 8 0	68	75	79	
12	S	Clear	30 0 0	69	76	78	
15	S W	Clear	30 1 7	69	75	77	
16	S W	Clear	30 1 3	75	80	83	Rain.
17	S W	Clear				79	Rain.
18	S W	Clear	30 1 2	78	80	83	
19	S W	Clear	30 0 0	78	82	85	
20	S W	Clear	30 0 0	75	82	85	
21	S W	Clear	30 0 4	71	80		
22	N E	Rain	30 0 3	70	70	71	
23	N by E	Clear	30 1 0	65	74	75	
24	E	Clear	30 0 3	64	72	80	
25	S W	Clear	29 9 0	75	81	85	
26	N	Clear	29 9 4	74	81		
27	W	Clear	29 8 0	75	81	82	Rain.
28	W	Clear	29 9 0	70	72	73	
29	N W	Clear	30 1 7	60	63	65	
30	W	Clear	30 3 0	60	62	63	
31	E by S	Cloudy	30 2 9	60	66	70	

JUNE,

JUNE, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Observations.
1	S E	Rainy	30 1 0	65	69	72	
2	N W	Cloudy	30 0 0	70	75	76	
3	N b E	Cloudy	30 0 0	70	74	75	
4	E	Clear	30 0 2	64	70	71	
5	E	Cloudy	30 0 2	64	70	71	
6	S E	Cloudy	30 0 0	65	75	78	
7	E	Cloudy	29 8 8		75	73	
8	N	Clear	29 8 7	65	74	75	
9	N	Clear	30 0 6		75	76	
11	W b S	Clear	29 9 9	70	80	84	
12	N E	Cloudy	29 8 0	70	73	74	Rain.
13	N E	Clear	29 9 0	68	73	72	
14	N E	Clear	29 9 6	64	65	69	
15	N E	Cloudy	30 0 3	68	70	72	
16	W	Clear	30 1 0	67	75	78	
17	S W	Clear	30 0 0	70.	80	84	
18	S W	Clear	29 9 5	78	87	86	
19	E	Rainy	29 9 3	78	79	79	
20	E	Rainy	29 9 0	79	79	79	
21	S E	Cloudy	29 9 7	75	83	86	Rain.
22	S W	Clear	29 9 9	76	83	87	Much Rain.
23	S b E	Cloudy	29 9 5	76	77	77	
24	S b E	Cloudy	29 9 2	75	80	81	
25	S W	Clear	29 9 7	78	84	86	
26	S W	Clear	30 0 0	78	85	87	
27	S W	Clear	30 0 0	80		86	
28	S W	Clear	29 9 5	80	87	86	
29	S W	Clear	29 9 9	75	86	88	
30	S W	Clear	30 0 0	80	86	89	

JULY,

JULY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S W	Clear	29 9 9	80	86	89	
2	W	Clear	29 9 0	70	84	80	
3	W	Clear	29 9 5	77	83	81	Rain.
4	N W	Cloudy	29 8 2	73	75	76	
5	E	Clear	30 0 5	73	76	79	
6	N E	Clear	30 1 6	75	77	81	
7	W	Clear	30 0 7	76	79	88	
8	S W	Clear	30 0 3	78	85	86	
9	S W	Clear	30 0 0	80	86	88	Rain.
10	S W	Clear	30 0 0	80			
11	S W	Clear	30 0 0	80	88	90	
12	S W	Clear	29 9 5	80	87	88	Rain.
13	E	Cloudy	29 9 7	77	80	81	
14	N E	Cloudy	30 0 0	70	73	75	
15	N W	Clear	30 2 0	71	75	77	
16	N E	Clear	30 2 5	73	75	78	
17	E	Cloudy	30 2 2	73	76	77	
21	N E	Cloudy	30 0 0	77	77	78	Rain.
22	E	Rain	29 8 8	75	76	76	Rain.
23	N E	Cloudy	29 9 0	69	73	75	
24	E	Clear	29 9 9	72	77	77	Rain.
25	E	Cloudy				77	Rain.
26	N E	Rain	30 1 7	73	75	77	Rain.
27	N E	Cloudy	30 1 6	73	75	78	Rain.
28	S W	Clear	30 0 7	75	82	85	Rain.
29	S W	Clear	30 0 0	75	83	86	
30	S W	Clear	30 0 0	78	85	87	
31	S W	Clear	30 0 0	78	85	87	

AUGUST, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Observations.
1	S W	Clear	30 0 0	79	86	90	
2	S W	Clear	30 0 0	80	81	89	
3	S W	Clear	29 9 7	78	84	86	
4	S W	Cloudy	29 9 6	75	77	79	
5	S W	Cloudy	30 0 5	75	75	81	
6	S W	Clear	30 1 3	76	79	82	
7	S W	Clear	30 1 9	76	80	83	
8	S E	Clear	30 2 0			85	
9	E	Cloudy	30 0 0	76		81	
10	S	Clear	30 0 2	77	78	72	
11	E	Rain	29 7 3	77	77	76	Much rain.
12	N W	Cloudy	29 8 0	72			
13				67	67	68	Rain.
14	S E	Cloudy	29 7 8	66	67	67	
15	S	Cloudy	29 9 0	68	70	75	
16	S	Cloudy	29 9 9	75	80	82	
17	S W	Cloudy	30 0 2	75			

Description

N° XI.

Deſcription of a Machine for Meaſuring a Ship's Way through the Sea, by F. Hopkinson, *Eſquire.*

Read July 11, 1783. THE errors and uncertainties incident to the mariner's log, in common uſe, are too obvious not to be univerſally acknowledged. Were it not for the obſervations navigators are accuſtomed to make of a ſhip's progreſs, by the apparent paſſage of the water along ſide, and the ſigns that uſually preſent on approaching the land, the log, alone, would be a very unſafe dependence.

Several attempts have, therefore, been made to improve the log, and render its indications more accurate, but without ſucceſs. All the machines of this kind, that I have heard of, were compoſed of a number of wheels, which were to be put in motion by the twiſting of a line let out aſtern, having a drag at the end ſo conſtructed as to whirl round, faſter or ſlower, according to the motion of the ſhip.

The objections to a machine, ſo conſtructed, are, *Firſt*, If the line is not very long the drag will be conſiderably affected by the ſhip's wake; and, if it is very long, the twiſtings will be irregular, and the line liable to kink. *Secondly*, If the drag is ſo heavy as to ſink below the bottoms of the waves, when the ſhip ſails faſt, it will be too heavy, and ſink too deep when ſhe hath but a ſlow progreſſive motion: Or, otherwiſe, if the drag is of a proper weight when ſhe makes but little way, it will ſkip from wave to wave through the air when ſhe ſails with a briſk gale, and ſo be of no uſe. And, *Thirdly*, It will be liable to moſt of the other irregularities to which the log in common uſe is expoſed.

The machine now propoſed will, it is hoped, be free from, at leaſt, ſome of theſe objections. And, although it may not be able to aſcertain a ſhip's way through the ſea to a mathematical preciſion, yet if it ſhould be found to anſwer the purpoſe better than any inſtrument hitherto contrived, it may be admitted as an acquiſition to the art of navigation.

This machine, in its moſt ſimple form, is repreſented by *Fig.* 1, *Plate* 3. Wherein AB is a ſtrong rod of iron moveable on the fulcrum C. D is a thin circular plate of braſs rivetted to the lower extremity of the rod. E an horizontal arm connected at one end with the top of the rod AB by a moveable joint F, and at the other end with the bottom of the index H by a like moveable joint G. H is the index turning on its centre I and travelling over the graduated arch K; and L is a ſtrong ſpring bearing againſt the rod AB and conſtantly counteracting the preſſure upon the palate D. The rod AB ſhould be applied cloſe to one ſide of the cut water or ſtem, and ſhould be of ſuch a length that the palate D may be no higher above the keel than is neceſſary to ſecure it from injury when the veſſel is aground or ſails in ſhoal water. As the bow of the ſhip curves inward towards the keel M, the palate D will be thrown to a diſtance from the bottom of the veſſel, although the perpendicular rod, to which it is annexed, lies cloſe to the bow above; and, therefore, the palate will be more fairly acted upon. The arm E ſhould enter the bow ſomewhere near the hawſer hole, and lead to any convenient place in the forecaſtle, where a ſmooth board or plate may be fixed, having the index H and graduated arch K upon it.

It is evident from the figure, that as the ſhip is urged forward by the wind, the palate D will be preſſed upon by the reſiſting medium, with a greater or leſs force according to the progreſſive motion of the ſhip: and this will operate upon the levers ſo as to immediately affect the index; making the leaſt encreaſe or diminution of the ſhip's way

way viſible on the graduated arch. The ſpring L always counteracting the preſſure upon the palate, and bringing back the index on any relaxation of the force impreſſed.

A ſhip going through the ſea opens a paſſage for herſelf, making a hole in the water equal to her immerged bulk. As ſhe paſſes on, this vacancy is filled up by the tumbling in of the waters from each ſide, and from underneath, at the ſtern, with great violence. So that there is a fair current of water from her bow to her ſtern, paſſing under the bottom and along ſide; the force of which current is in direct proportion to the velocity of the ſhip's progreſſive motion. This machine is, therefore, advantageouſly placed at the bow of the ſhip, where the current firſt begins, and acts fairly upon the palate; in preference to the ſtern, where the tumultuous cloſing of the waters cauſes a wake, viſible to a great diſtance. The palate D is ſunk nearly as low as the keel, that it may not be influenced by the heaping up of the water, and the daſhing of the waves at and near the water-line. The arch K is to aſcertain how many knots or miles ſhe would run in one hour, at her then rate of ſailing. But the graduations on this arch muſt be unequal; becauſe the reſiſtance of the ſpring L will encreaſe as it becomes more bent; ſo that the index will travel over a greater ſpace from one to five miles (which I ſuppoſe to be a medium) than from five to twelve. Laſtly, the palate, rod, ſpring and all the metallic parts of the inſtrument ſhould be covered with a ſtrong varniſh, to prevent ruſt from the corroſive quality of the ſalt water and ſea air.

IMPROVEMENT OF THIS MACHINE.

Let the rod or ſpear AB *Fig.* 1, be a round rod of iron or ſteel; and inſtead of moving on the fulcrum or joint as at C, let it paſs through and turn freely in a ſocket, to which ſocket the moveable joint muſt be annexed as, repreſented

preſented in *Fig.* 2. The rod muſt have a ſhoulder to bear on the upper edge of the ſocket, to prevent its ſlipping quite down. The rod muſt alſo paſs through a like ſocket at F, *Fig.* 1. The joint of the lower ſocket muſt be fixed to the bow of the ſhip, and the upper joint or ſocket muſt be connected with the horizontal arm E. On the top of the uppermoſt ſocket, let there be a ſmall circular plate, bearing the 32 points of the mariner's compaſs; and let the top of the rod AB come through the centre of this plate, ſo as to carry a ſmall index upon it, as is repreſented in *Fig.* 3. This ſmall index muſt be fixed to the top of the rod on a ſquare; ſo, that by turning the index round the plate, the rod may alſo turn in the ſockets, and of courſe carry the palate D round with it. The little index always pointing in a direction with the face of the palate. The ſmall compaſs plate ſhould not be faſtened to the top of the ſocket, but only fitted tightly on, that it may be moveable at pleaſure. Suppoſe, then, the intended port to bear ſouth-weſt from the place of departure; the palate muſt be turned on the ſocket till the ſouth-weſt point thereon looks directly to the ſhip's bow; ſo that the ſouth-weſt and north-eaſt line on the compaſs plate may be preciſely parallel with the ſhip's keel; and in this poſition the plate muſt remain during the whole voyage. Suppoſe then the ſhip to be ſailing in the direct courſe of her intended voyage, with her bowſprit pointing ſouth-weſt, let the little index be brought to the ſouth-weſt point on the compaſs plate, and the palate D will neceſſarily preſent its broad face toward the port of deſtination; and this it muſt always be made to do, be the ſhip's ſailing courſe what it may. If, on account of unfavourable winds, the ſhip is obliged to deviate from her intended courſe, the little index muſt be moved ſo many points from the ſouth-weſt line of the compaſs plate, as the compaſs in the binnacle ſhall ſhew that ſhe deviates from her true courſe. So that, in whatever direction the ſhip ſhall ſail, the palate D will always

always look full to the ſouth-weſt point of the horizon, or towards the port of deſtination; and, conſequently, will preſent only an oblique ſurface to the reſiſting medium—more or leſs oblique as the ſhip deviates more or leſs from the true courſe of her voyage. As, therefore, the reſiſtance of the water will operate leſs upon the palate in an oblique than in a direct poſition, in exact proportion to its obliquity, the Index H will not ſhew how many knots the veſſel runs in her then courſe, but will (it is expected) indicate how many ſhe gains in the direct line of her intended voyage. Thus, in *Fig.* 5, if the ſhip's courſe lies in the direction of the line AB, but ſhe can ſail by the wind no nearer than AC; ſuppoſe then, her progreſſive motion ſuch as to perform AC, equal to five knots or miles in one hour; yet the index H will only point to four knots on the graduated arch, becauſe ſhe gains no more than at that rate on the true line of her voyage, viz. from A to B. Thus will the difference between her real motion and that pointed out by the index be always in proportion to her deviation from the intended port, until ſhe ſails in a line at right angles therewith, as AD; in which caſe the palate would preſent only a thin ſharp edge to the reſiſting medium; the preſſure of which ſhould not be ſufficient to overcome the friction of the machine, and the bearing of the ſpring L. So that at whatever rate the ſhip may ſail on that line yet the index will not be affected: Shewing that ſhe gains nothing on her true courſe. In this caſe, and alſo when the veſſel is not under way, the action of the ſpring L ſhould cauſe the index to point at 0; as repreſented by the dotted lines in figure 1 and 4.

As the truth of this inſtrument muſt depend on the equal preſſure of the reſiſting medium upon the palate D according to the ſhip's velocity, and the proportionable action of the ſpring L, there ſhould be a pin or ſcrew at the joints C and F, ſo that the rod may be readily unſhipped and taken in, in order to clean the palate from any foulneſs

foulneſs it may contract; which would greatly increaſe its operation on the index H, and thereby render the graduated arch, falſe and uncertain.

Further, the ſpring L may be expoſed too much to injury from the ſalt water, if fixed on the outſide of the ſhip's bow. To remedy this it may be brought under cover by conſtructing the machine as repreſented by figure 4. Where, A B is the rod, C the fulcrum or centre of its motion; D the palate; E the horizontal arm leading through a ſmall hole into the forecaſtle: M is a ſtrong chain, faſtened at one end to the arm E and at the other to a rim or barrel on the wheel G, which by means of its teeth gives motion to the ſemicircle I and index H. The ſpring L is ſpiral and encloſed in a box or barrel, like the main ſpring of a watch: A ſmall chain is fixed to and paſſing round the barrel is faſtened by the other end to the fuzee W. This fuzee is connected by its teeth with the wheel G, and counteracts the motion of the palate D. NN are the two ſockets through which the rod A B paſſes, and in which it is turned round by means of the little index R. S is the ſmall compaſs plate, moveable on the top of the upper ſocket N. The plate S hath an upright rim round its edge cut into teeth or notches; ſo that when the index R is a little raiſed up, in order to bring it round to any intended point, it may fall into one of theſe notches and be detained there: Otherwiſe the preſſure of the water will force the palate D from its oblique poſition, and turn the rod and index round to the direction in which the ſhip ſhall be then ſailing. Should it be apprehended that the palate D, being placed ſo far forward, may affect the ſhip's ſteerage or obſtruct her ſailing, it ſhould be conſidered that a very ſmall plate will be ſufficient to work the machine. I ſhould ſuppoſe that one of three or four inches in diameter would fully anſwer the purpoſe: And yet not be large enough to have any ſenſible operation on the helm or ſhip's way.

The

The greateſt difficulty, perhaps, will be in graduating the arch K; (if the machine is conſtructed as in *figure* 1.) the unequal diviſions of which can only be aſcertained by actual experiment on board of each ſhip reſpectively; in as much as the accuracy of theſe graduations will depend on three circumſtances, viz. The poſition of the fulcrum C with reſpect to the length of the rod, the ſize of the palate D and the ſtrength or bearing of the ſpring L. When theſe graduations, however, are once aſcertained for the machine on board of any one veſſel, they will not want any future alterations; provided the palate D be kept clean, and the ſpring L retains its elaſticity.

But the unequal diviſions of the graduated arch will be unneceſſary, if the machine is conſtructed as in *figure* 4. For as the chain goes round the barrel L, and then winds through the ſpiral channel of the fuzee W, the force of the main-ſpring muſt operate equally, or nearly ſo, in all poſitions of the index; and conſequently, the diviſions of the arch K may, in ſuch caſe, be equal.

After all, it is not expected that a ſhip's longitude can be determined to a mathematical certainty by this inſtrument. The irregular motions and impulſes to which a ſhip is continually expoſed, make ſuch an accuracy unattainable perhaps by any machinery: But if it ſhould be found, as I flatter myſelf it will on fair experiment, that it anſwers the purpoſe much better than the common log, it may be conſidered as an acquiſition to the art of navigation.

It ſhould be obſerved that in aſcertaining a ſhip's longitude by a time-piece, this great inconvenience occurs, that a ſmall and trifling miſtake in the time, makes a very great and dangerous error in the diſtance run: Whereas the errors of this machine will operate no farther than their real amount; which can never be great or dangerous, if corrected by the uſual obſervations made by mariners for correcting the common log.

A like machine made in its ſimple form, (as at fig. 1.) ſo conſtructed as to ſhip and unſhip, might occaſionally

be applied along ſide about midſhips in order to aſcertain the lee-way; which, if rightly ſhewn will give the ſhip's preciſe longitude. As to ſea-currents, this and all other machines hitherto invented, muſt be ſubject to their influence; and proper allowances muſt be made, according to the ſkill and knowledge of the navigator.

Laſtly, ſome diſcretion will be neceſſary in taking obſervations from the machine to be entered on the log-book. I mean, that the moſt favourable and equitable moment ſhould be choſen for the obſervation. Not whilſt the ſhip is rapidly deſcending the declivity of a wave; or is ſuddenly checked by a ſtroke of the ſea; or is in the very act of plunging. In all caſes, I ſuppoſe, periods may be found in which a ſhip proceeds with a true average velocity; to diſcover which a little experience and attention will lead the ſkilful mariner*.

N° XII.

Account of an Electrical Eel, or the Torpedo of Surinam, by WILLIAM BRYANT, *Eſquire.*

SURINAM a colony of South America belonging to the ſtates of Holland, abounds with as many natural curioſities as any country in the world. But that which I look upon to be as ſurpriſing as any in it, and which I believe has not yet been accurately deſcribed, is a fiſh of the ſpecies of eel, and is caught there in nets among other fiſh; generally in muddy rivers, and I believe is found in moſt of the neighbouring provinces. In ſize and colour

* An ingenious mechanic would probably conſtruct this machine to better advantage in many reſpects. The author only meant to ſuggeſt the principle; experiment alone can point out the beſt method of applying it. He is ſenſible of at leaſt one deficiency, viz. That the little index R, figure 4, will not be ſtrong enough to retain the palate D in an oblique poſition when the ſhip is ſailing by the wind; more eſpecially as the compaſs plate S, in whoſe notched rim the index R is to fall, is not fixed to, but only fitted tight on the ſocket N. Many means however might be contrived to remedy this inconvenience.

colour it is not unlike a common eel of Europe or America, and in ſhape reſembles it more, except that it is thicker in proportion to its length, and the head is more flat and not ſo pointed; but differs from them in this reſpect, that it comes to the ſurface to breathe in the air. It is called by the Dutch *Beave Aal*, and by the Engliſh inhabitants the Numbing Eel. As to the other qualities, of which I mean chiefly to take notice, and which I think are as different from the Torpedo of Europe, as the fiſh is in ſhape, they are as follows.

On touching the fiſh as it lies in the water in a tub provided for it, a ſudden and violent ſhock is received, in all reſpects like that which is felt on touching the prime conductor, when charged with the electrical fluid from the globe; and like that chiefly, affects the ends of the fingers and elbow. Gently holding the tail of the fiſh with one hand and touching the head with the other, a very violent ſhock is felt in both elbows and through the breaſt and ſhoulders. I at firſt imagined that the violence of the ſhock proceeded from both arms receiving it at the ſame time, and that the pain was no more than that of the two ſtrokes added together; but I found myſelf miſtaken. For upon ſeven perſons joining hands, and the firſt taking hold of the tail (which may with more eaſe be held than the head) and the ſeventh at the ſame time touching the head, we were all affected in both elbows, and that in the ſame manner as I remember to have been in the electrical experiment, when ſeveral perſons take hold of the wire and the equilibrium is reſtored by the fluids paſſing through their bodies.

I find the ſhock may be received through metallic ſubſtances. On touching the fiſh with an old ſword blade I was ſtrongly affected. But arming it with ſealing-wax and taking hold of that part which was covered with it, the electrical fluid (I cannot help calling it ſo) would not paſs. Neither has it any effect on the body when touched with

glaſs bottle, ſealing-wax, &c. Yet I cannot obſerve the leaſt diminution of this quality by placing the tub which contains the fiſh on glaſs bottles; it continues the ſame in all reſpects. So that whether it has an unaccountable faculty of collecting a quantity of the fluid from the ſurrounding waters, or through the body of the perſon touching it, or has in its own body a large fund which it can diſcharge at pleaſure, I am greatly at a loſs to think or imagine.

Although it has no effect on the human body when touched with a piece of wood, or indeed any other ſubſtance not metallic; yet an accident diſcovered to me, that on ſome occaſions the effect would be ſenſible through wood. For one morning while I was ſtanding by, as a ſervant was emptying the tub, which he had lifted intirely from the ground, and was pouring off the water to renew it, and the fiſh left almoſt dry, the negro received ſo violent a ſhock as occaſioned him to let the tub fall, and calling another to his aſſiſtance, I cauſed them both to lift the tub free from the ground, when pouring off the remains of the water they both received ſmart ſhocks and were obliged to deſiſt from emptying the tub in that manner. This I afterwards tried myſelf and received the like ſhock. This fiſh indeed was one of the largeſt I have ſeen and but newly caught. For I obſerve that after being ſometime confined in a tub and wanting perhaps their natural food, they loſe much of the ſtrength of this extraordinary quality. I am ſometimes apt to conjecture, that this animal has the power of communicating the ſtroke when, and with what degree of force it will; and that it ſerves him as a weapon of defence againſt his enemies. For I have often obſerved that on firſt taking hold of it, the ſhock is tolerable; but as ſoon as he perceives himſelf the leaſt confined, it is much more violent. This I experienced to my coſt, as I one day took hold of it, about the middle of the fiſh, I lifted it partly out of the water, when on a ſudden I received ſo ſmart a ſhock

that

that it occaſioned a ſtrong contraction in the bending muſcles of my fingers, and I could not immediately let it go; but endeavouring to diſengage my hand threw it on the ground; taking hold of it a ſecond time, to return it into the tub, I was more ſtrongly affected than at firſt, and that not only in my hands and arms, but throughout my whole body; the forepart of my head and the back part of my legs ſuffered principally; and in the ſame manner as on receiving a very ſmart ſhock from a highly charged phial in electrical experiments.

On obſerving that the ſenſation occaſioned by the ſhock as to the nature and degree of ſtrength upon touching different parts of the fiſh, was different, I was at firſt inclined to think it might be owing to its having an extraordinary faculty of containing more of the fluid in one part of its body than in another. The tail part to above one third of its length, occaſions rather a numbneſs and tingling, than pain, but on applying the end of the fingers to the back, head, and under part of its body, it cauſes a ſharp pricking pain. This may poſſibly be accounted for by the difference in the texture of the ſurface of the ſkin, as the manner of the electrical fluids coming from a glaſs tube is different when its ſurface is altered by being rubbed with different ſubſtances, as has been lately taken notice of in a letter to the Royal Society.

Theſe are the principal obſervations, the ſhort time I reſided at Surinam, allowed me an opportunity of making relating to this extraordinary animal.

Obſervations

N° XIII.

Obſervations on the Numb Fiſh, or Torporific Eel, by Henry Collins Flagg, *South-Carolina.*

Read March 7th, 1783.

I DO myſelf the pleaſure, though late I confeſs, to comply with my promiſe of communicating ſome obſervations on the Numb Fiſh, or *Torporific Eel*, which I think a more proper name. Theſe obſervations are contained in two letters I had the honor to write to the Rev. Dr. Stiles, a member of your philoſophical ſociety, from Rio Eſſequebo. Pleaſe to accept the following extracts.

The apparent difference between the torporific eel and that uſually caught in your harbour is, the former is flatter on the back and head, the upper part of which is perforated with ſeveral holes*, and has on each ſide, behind, a ſmall fin which ſome ſay are elevated or depreſſed as the fiſh is pleaſed or not†; the body I think is larger in proportion to the length, and it has a broad fin connected to the belly and continued to the tail. I have ſeen this fiſh four feet long. The ſenſation occaſioned by touching it appeared to me exactly ſimilar to an electric ſhock. I have as yet been able to procure only one of theſe eels, and that was injured by laying too long dry before it came to me. The following are the remarks I made the little time it lived. I received the fiſh from a negro in a wicker baſket, and laying it on the ground felt a conſiderable ſhock, as I did too when I turned the fiſh out of the baſket into a tub of

* Theſe holes do not penetrate to the mouth, nor could I diſcover the uſe of them. But I was not ſufficiently exact in my diſſection of the head, or I think I might have found the termination of theſe ducts.

† This is true.

of water. The ſhock is greater if the fiſh is enraged; but whether repeated touches will exhauſt this ſtrange power, as frequently repeated bites do the viperine and ſome other poiſons for a time, I believe no experiment has yet determined*. If a perſon hold his finger in the water ſeveral inches diſtant from the fiſh and another touch it, a ſhock equally ſevere is felt by him who does not touch it. The ſame thing happens if the fiſh exerts itſelf without being touched. If a number of perſons join hands, and one touch the eel, they are all equally ſhocked, unleſs there ſhould happen to be one of the number incapable of being affected by the eel, which is the caſe of a very worthy lady of my acquaintance, who can handle this fiſh at will. I am informed ſome Indians and negroes can do the ſame; whether by the aſſiſtance of any means to counteract the power of the eel, I know not; but am perſuaded it is ſomething in the conſtitution of the lady†. The eel I had obtained got out of the tub, and it was with ſome difficulty I returned it, for the repeated ſhocks I received through a piece of deal board eighteen inches long, with which I attempted to lift it, made my arms ache very much, and for a conſiderable time. I think the numbneſs occaſioned by touching this eel continues longer than that from an electric ſhock of the ſame degree of force, and I have been aſſured by a perſon of good ſenſe and veracity, that a negro fellow formerly being bantered by his companions for his fear of this eel, determined to give a proof of his reſolution, and attempted to graſp it with both hands. The unhappy conſequence was, a confirmed paralyſis of both arms. I hear this fellow is ſtill living in the iſland of St. Chriſtopher's; if ſo, I can obtain more ſatisfaction, for I have my doubts of the negro's honeſty‡. But very certain

* I am ſince convinced they do.

† This lady, when I became acquainted with her, was far gone in an hectic fever. And I did not think to enquire if ſhe could treat the fiſh with ſo much familiarity while in a perfect ſtate of health.

‡ This account was afterwards confirmed to me, with the further information, that after ſeveral years the negro recovered the uſe of his arms by ſlow degrees, and I think without any aſſiſtance from medicine.

tain it is, that many perſons have been knocked down by the ſeverity of the ſhock. The languid ſtate in which I found the eel the morning after it was taken, gave me an opportunity of obſerving that though I could perceive no ſhock by touching it on or near the tail; yet applying my finger near the belly, the torporific power was very conſiderable, notwithſtanding the fiſh was now almoſt dead. This I repeated ſeveral times, as a remark of ſome conſequence in aſſiſting us to determine whether, or how far, the emiſſion of torporific particles depends on the exertion of any muſcular force*; upon which principle Mr. Reamure accounts for the benumbing power of the Torpedo. I much doubt if the moſt acute eye can diſcern any motion in the eel at the time it ſhocks†. I have been ſo particular in taking notice of the baſket and deal-board, becauſe it has been aſſerted that the eel ſhocks only by immediate contact, through metal or very hard wood. This eel is frequently eat by the negroes, and reckoned very delicious. Its common food is ſhrimps or any ſmall fiſh.

I have lately made another experiment upon the torporific eel. It was ſuggeſted to me by the very great ſimilarity between the effects of a ſhock from the eel and an electric machine. I held an iron rod between two pieces of glaſs and touched the eel with it, but could not perceive the leaſt ſhock. I held the rod in a ſilk handkerchief with the ſame effect. I repeated theſe experiments on two eels with equal ſucceſs. I think this experiment demonſtrates that the electric and torporific particles are the ſame. I have tried the effects of this fiſh upon the needle of a compaſs but perceived no influence. I have not, however, done

* I have not ventured as yet to give any opinion of the ſtrange property by which this fiſh becomes the conductor of the electric fluid. But that the emiſſion of it depends upon the exertion of muſcular force may, I think, be concluded from hence; that, as has already been determined, repeated exertions will exhauſt its power to ſhock for a time, and before it can again exert its influence, a freſh quantity of fire muſt be collected; nor do I think the experiment I made on the dying eel invalidates this opinion, for to the beſt of my recollection it ceaſed to ſhock ſome time before its death.

† I am informed the motion is perceptible, though I confeſs I could not diſtinguiſh it.

done with the eel, and hereafter will repeat all the old and make new experiments upon it*.

This fiſh raiſes its head every few minutes above the water to reſpire.

I have ſeen negroes take hold of it, at firſt very cautiouſly, receiving many light ſhocks, but preſently have graſped it hard and taken it out of the water.

There is a kind of light wood through which the eel cannot ſhock.

Mrs. Behn, in her Oroonoko, gives a deſcription of this fiſh, which ſhe calls the numb-eel, and ſays it is taken in the river Surinam.

From the above experiments, partial as they are, I leave you, ſir, to judge how far the torporific and electric fluids are alike.

I am, with the greateſt reſpect and eſteem,

Your moſt humble ſervant,

South-Carolina, October 8, 1782. } HENRY COLLINS FLAGG.

N° XIV.

To DAVID RITTENHOUSE, *Eſquire, from* JOHN PAGE, *Eſquire.*

Williamſburg, December 4, 1779.

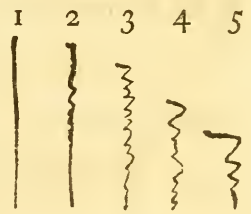
DEAR SIR,

Read May 2d, 1783. I HAVE often thought there was a ſtrong reſemblance between ſome of the phenomena of electricity and magnetiſm, and fancied I ſaw ſomething like the two electricities in the attraction and repulſion of

 the

* I had not been long in South-America when I made my obſervations; ſoon after which, the neceſſary avocations of my profeſſion, together with that relaxation of the mental powers generally conſequent upon the laſſitude of body incident to the inhabitants of warm climates, indiſpoſed me to the farther proſecution of experiments I am now mortified at not having made.

the two poles. I have amuſed myſelf with ſuppoſing that magnetiſm is only a ſpecies of electricity, whoſe *matter* is as yet not diſcovered by human ſight; as that of electricity was, when a few years ago, it was perceivable only by its effects in attracting or repelling light bodies, as magnetiſm now is in attracting or repelling iron. Experiments by which polarity may be given to needles by means of electricity, perhaps, further improved and cloſely attended to, might throw great light on this ſubject. I wiſh we had more caſes ſtated of the effects of lightening and the Aurora Borealis on the needle. But mentioning the Aurora Borealis recalls to my mind, the meteor which was ſeen at many diſtant places in Virginia on the 31ſt of October, at about 6^h 10^m P. M. It was what is vulgarly called a falling ſtar. It fell as ſeen at Roſewell about three or four degrees to the north of weſt and left a bright trail of light behind it; which extended from the horizon perpendicularly above 7°; unluckily I loſt a view of it when falling, but was called out time enough to ſee the grand and beautiful appearance of its trail of light. It was ſeen for near 15^m, it was as bright as ſhining ſilver, and as broad as the enlightened part of the new moon, when firſt viſible, and about 7° in length. It might be repreſented by N° 1, when I firſt ſaw it, and by the other figures at intervals of about a minute after. Juſt before it diſappeared it reſembled the edge of a cloud. The ſky was remarkably clear and ſerene. It appeared in the ſame manner exactly to ſeveral gentlemen above an hundred miles from Roſewell, but on a different point of the compaſs. I have not yet had ſo accurate an account of its bearing as to aſcertain its height and diſtance. Did you ſee any thing of it?



I am, dear ſir, yours moſt ſincerely,

JOHN PAGE.

From DAVID RITTENHOUSE, *Eſquire*, *to* JOHN PAGE, *Eſquire*.

Philadelphia, January 16, 1780.

DEAR SIR,

Read May 2, 1783. I DESIGN to give you my thoughts on Magnetiſm in ſome future letter, at preſent I ſhall confine myſelf to the ſubject of the latter part of yours of the 4th of December laſt.

The extraordinary Meteor you mention was likewiſe viſible here, the air being ſerene and clear. I did not ſee it until the bright ſtreak was become very crooked, it then bore S. 70° W. nearly, from Philadelphia, and comparing this courſe with that obſerved by you, I find it muſt have fallen on or near the Ouaſiota mountains mentioned in Lewis Evans's map, about 480 miles from Philadelphia and 365 from Williamſburg. And taking its altitude 7,° as obſerved by you, adding $2\frac{1}{2}$ degrees for the depreſſion of that place below your horizon, its entire apparent altitude above the ſpot where it fell was $9°\frac{1}{2}$, which, on a radius of 365 miles, will be 61 miles perpendicular height. The breadth of the luminous vapour was, I think, in ſome places, when I ſaw it, not leſs than a quarter of a degree; this at 480 miles diſtance muſt have been at leaſt two miles. It was certainly a grand appearance near the place where it fell, if any human eye was there.

May not theſe ſhooting ſtars be bodies altogether foreign to the earth and its atmoſphere, accidentally meeting with it as they are ſwiftly traverſing the great void of ſpace? And may they not, either electrically or by ſome other means, excite a luminous appearance on entering our atmoſphere? I am inclined to this opinion for the following reaſons: 1ſt. It is not probable that meteors ſhould be generated in the air at the height of 50 or 60 miles, on account of its extreme rareneſs; and many falling ſtars, beſides this, are known with certainty to have been at very

great heights. 2dly. Their motions cannot be owing to gravity, for they deſcend in all directions, and but ſeldom perpendicularly to the horizon. Beſides, their velocities are much too great. This meteor would not have fallen by the force of gravity, from the place where it firſt appeared, to the earth, in leſs than two minutes of time; nor in leſs than ten ſeconds, if we ſuppoſe it impelled by gravity from the remoteſt diſtance. They are nevertheleſs affected by gravity in ſome manner, for I cannot find that any one was ever obſerved to aſcend upwards in its courſe.

It is true that difficulties will likewiſe occur, if we ſuppoſe them to be foreign bodies of ſufficient denſity to preſerve ſuch great degrees of velocity even in paſſing through the atmoſphere, for it may be aſked why do they not frequently ſtrike the earth, buildings, &c.

Perhaps they are generally, if not always, exploded in paſſing through the air, ſomething in the manner that filings of ſteel are exploded in paſſing through the flame of a candle. And at the ſame time that they afford us occaſion to admire the variety and immenſity of the Creator's works, they may perhaps produce ſome important and neceſſary effects in the atmoſphere ſurrounding this globe, for the welfare of man and its other innumerable tribes of inhabitants.

I am, dear ſir, your affectionate friend,

And very humble ſervant,

DAVID RITTENHOUSE.

Deſcription

N° XV.

Deſcription of the Grotto at Swatara, by the Rev. PETER MILLER, *of Ephrata; communicated by* WILLIAM BARTON, *Eſquire.*

Read March 7, 1783.

"AS the courſe of my letter now tends this way, I muſt remind you, if ever you ſhould publiſh a natural hiſtory of Pennſylvania, not to conſign to oblivion that very curious petrifying cavern, of which, leſt you ſhould not have ſeen it already, I ſhall give ſome deſcription.

"It is ſituate on the eaſt ſide of Swatara, cloſe to the river. Its entrance is very ſpacious, and there is ſomewhat of a deſcent towards the other extremity; inſomuch that I ſuppoſe the ſurface of the river is rather higher than the bottom of the cave. The upper part is like an arched roof, of ſolid lime-ſtone rock, perhaps twenty feet thick. On entering, are found many apartments, ſome of them very high, like the choir of a church. There is, as it were, a continual rain within the cave, for the water drops inceſſantly from the roof upon the floor; by which, and the water petrifying as it falls, pillars are gradually formed to ſupport the roof. I ſaw this cave about thirty years ago, and obſerved above ten ſuch pillars, each ſix inches in diameter and ſix feet high; all ſo ranged that the place incloſed by them reſembled a ſanctuary in a Roman church: And I can aſſure you, that no royal throne ever exhibited more grandeur, than the delightful proſpect of this *luſus naturæ*. Satisfied with the view of this, we diſcovered the reſemblances of ſeveral monuments, incorporated into the walls, as if the bodies of departed heroes were there depoſited. Our guide then conducted us to a place, where, he

he ſaid, hung the bell: This is a piece of ſtone iſſuing out of the roof, which when ſtruck ſounds like a bell.

"Some of the ſtalactites are of a colour like ſugar-candy, and others reſemble loaf-ſugar; but it is a pity that their beauty is now almoſt deſtroyed by the country people. The water, as it falls, runs down the declivity; and it is both wholeſome and pleaſant to drink, when it has diſcharged its petrifying matter. It is remarkable that we found ſeveral holes at the bottom of the cave, going down perpendicularly, perhaps into the abyſs, which renders it dangerous to be without a light. At the end of the cave, there is a pretty run, which takes its courſe through part of it, and then loſes itſelf among the rocks: Here is alſo its exit, by an aperture which is very narrow. Through this the vapours continually paſs outwards, with a ſtrong current of air; and, at night, theſe vapours aſcending reſemble a great furnace. Part of theſe vapours and fogs appear, on aſcending, to be condenſed at the head of this great alembic, and the more volatile parts to be carried off, through the aperture communicating with the exterior air before mentioned, by the force of the air in its paſſage.

"I beg pardon for having troubled you with ſuch a long detail. It appears ſtrange to me that none of our philoſophers have hitherto publiſhed a true account of this remarkable grotto."

N° XVI.

An Account of ſome Experiments on Magnetiſm, in a Letter to JOHN PAGE, *Eſquire, at Williamsburg.*

DEAR SIR,

Read Feb. 6, 1781. AGREEABLE to the promiſe in my laſt, I ſhall now communicate to you ſome conjectures and experiments on magnetiſm, which may perhaps either

either afford you ſome amuſement, or induce you to purſue the ſubject to more certainty.

I ſuppoſe then, that magnetical particles of matter are a neceſſary conſtituent part of that metal which we call iron, though they are probably but a ſmall proportion of the whole maſs. Theſe magnetical particles I ſuppoſe have each a north and a ſouth pole, and that they retain their polarity, however the metal may be fuſed or otherwiſe wrought. In a piece of iron which ſhews no ſigns of magnetiſm theſe magnetical particles lie irregularly, with their poles pointing in all poſſible directions, they therefore mutually deſtroy each other's effects. By giving magnetiſm to a piece of iron we do nothing more than arrange theſe particles, and when this is done it depends on the temper and ſituation of the iron whether that arrangement ſhall continue, that is, whether the piece of metal ſhall remain for a long time magnetical or not.

There is ſome power, whenceſoever derived, diffuſed through every part of ſpace which we have acceſs to, which acts on theſe magnetical particles, impelling one of their poles in a certain direction with reſpect to the earth and the other pole in the oppoſite direction. The direction in which this power acts I take to be the ſame with that of the dipping needle.

By applying a magnet to a piece of iron it becomes magnetical; for the magnet acting ſtrongly on the above mentioned particles, that action arranges them properly; overcoming the reſiſtance of the ſurrounding parts of the iron, and this reſiſtance afterwards ſerves to ſecure them in their proper ſituations, and prevents their being deranged by any little accident.

If we place a piece of iron in or near the direction of the dipping needle, it will in time become magnetical; that general power producing in this caſe the ſame effect as the application of the magnet, though in a weaker degree.

Iron

Iron or ſoft ſteel receives magnetiſm more eaſily than hardened ſteel, but will not retain it; may not this be, becauſe the magnetical particles are not ſo cloſely confined in ſoft as in hardened ſteel, and on that account more caſily admit of arrangement or derangement. By making a piece of ſteel red hot, or by twiſting it or beating it with a hammer, we may effectually deſtroy its magnetiſm. Now all theſe operations certainly derange the particles which compoſe the bar. By rubbing one piece of ſteel with another, magnetiſm may be produced, and it is eaſy to conceive how this operation, by the tremulous motion which it excites, may contribute to arrange the magnetical particles.

We took a ſoft ſteel ramrod, which did not diſcover the leaſt ſign of magnetiſm, and holding it in the direction of the dipping needle, ſtruck it ſeveral ſmart blows with a hammer, on one end; then laying it on a watch chryſtal it traverſed very well; that end which was held downwards, when ſtruck, becoming a north pole, whether the ſtroke was applied to the upper or the lower end. By turning the ſouth end downwards and ſtriking it afreſh, the magnetiſm was deſtroyed or reverſed, and it was curious to obſerve how very nicely you muſt adjuſt the number and force of the ſtrokes, preciſely to deſtroy the magnetiſm before communicated, without giving it anew, in a contrary direction. When we held the ramrod directly acroſs the line of the dipping needle, whilſt it was ſtruck with a hammer, on many trials it did not diſcover any ſigns of magnetiſm. But when held in any other direction, that end which approached neareſt to the point which the lower end of the dipping needle tends to, always became the north pole. From all this does it not ſeem very probable that during the concuſſion of the ſtroke, and whilſt the magnetical particles of the rod were moſt diſengaged from the ſurrounding matter, the active power abovementioned ſeized them and arranged them properly, where being confined,

confined, the rod afterwards remained magnetical. All this is nevertheleſs little more than conjecture, until confirmed by further experiments.

I am, dear ſir, yours, &c.

DAVID RITTENHOUSE.

N° XVII.

New Method of placing a Meridian Mark, in a Letter to the Rev. Dr. EWING, *Provoſt of the Univerſity. By* D. RITTENHOUSE, *Eſquire.*

DEAR SIR,

Read Nov. 1785. SOME time ago I mentioned to you a new invention I had for fixing a Meridian Mark for my Obſervatory. This I have ſince executed, and as it anſwers perfectly well, I ſhall give you a particular deſcription of it.

When my obſervatory was firſt erected, I placed a meridian mark to the northward at the diſtance of about 1200 feet, my view to the ſouth being too much confined by adjacent buildings, and that to the north was not diſtant enough to have the mark free from a ſenſible parallax. But laſt ſummer a new brick houſe was built directly north of the obſervatory, and much too nigh for diſtant viſion with the tranſit inſtrument. Now though a fixed mark is not abſolutely neceſſary where you have a good tranſit inſtrument, the poſition of which may be examined and accurately corrected, if neceſſary, every fair day, by the paſſage of the pole-ſtar above and below the pole, it is nevertheleſs very convenient, ſaves much trouble, and may ſometimes prevent miſtakes. We have an inſtance in the obſervations of the Aſtronomer Royal at Greenwich. His

mark being taken down at repairing the building to which it was ſecured, the tranſit inſtrument was accidentally thrown out of its true poſition, and the obſervations with it were continued for a conſiderable time before the error was detected. My meridian mark being thus rendered uſeleſs, I contrived ſeveral other methods of ſupplying its place, all of which were, on ſufficient deliberation, rejected for the following.

I faſtened the object glaſs of a thirty ſix feet teleſcope, firmly, to the wall which ſupports the tranſit inſtrument, oppoſite to and as near as convenient to the object glaſs of the tranſit, when brought to a horizontal ſituation. In the focus of the thirty ſix feet object glaſs I ſcrewed faſt a piece of braſs to a block of marble, ſupported by a brick pillar built on a good foundation, for this purpoſe, in my garden. On this piece of braſs are ſeveral black concentric circles; the reſt of the plate is ſilvered. The diverging rays of light which proceed from every point in theſe circles, after paſſing through the thirty-ſix feet glaſs become parallel, and entering the tranſit inſtrument, an image of the plate and its circles is formed in the ſame place where the images of ſtars or the moſt diſtant objects are formed. The circles are therefore diſtinctly ſeen through the tranſit, and being placed in the ſame meridian with the centre of the thirty ſix feet glaſs, the innermoſt circle, about the ſize of a brevier ○, ſerves for a meridian mark, to the centre whereof the croſs hair of the tranſit may be nicely adjuſted.

This mark is in ſeveral reſpects preferable to one placed in the common way. It is entirely free from parallax, which the other cannot be, unleſs placed at a very great diſtance, when glaſſes of great magnifying powers are uſed. It is not ſenſibly affected by the undulation of the air, which very often renders it impoſſible to ſet the tranſit accurately to a diſtant mark. And it can be illuminated at night without difficulty, ſhould the ſuſpicion of any accident

accident to the tranſit make it neceſſary. But it has likewiſe one diſadvantage. Should the pillar in ſettling, carry the mark a little to the eaſt or weſt, the error will be greater in proportion to its nearneſs.

I am, dear ſir, your humble ſervant,

DAVID RITTENHOUSE.

P. S. The great improvement of object glaſſes by Dolland has enabled us to apply eye glaſſes of ſo ſhort a focus, that it is difficult to find any ſubſtance proper for the croſs hairs of fixed inſtruments. For ſome years paſt I have uſed a ſingle filament of ſilk, without knowing that the ſame was made uſe of by the European aſtronomers, as I have lately found it is by Mr. Hirſchell. But this ſubſtance, though far better than wires or hairs of any kind, is ſtill much too coarſe for ſome obſervations. A ſingle filament of ſilk will totally obſcure a ſmall ſtar, and that for ſeveral ſeconds of time, if the ſtar be near the pole. I have lately with no ſmall difficulty placed the thread of a ſpider in ſome of my inſtruments, it has a beautiful effect, it is not one tenth of the ſize of the thread of the ſilkworm, and is rounder and more evenly of a thickneſs. I have hitherto found no inconvenience from the uſe of it, and believe it will be laſting, it being more than four months ſince I firſt put it in my tranſit teleſcope, and it continues fully extended, and free from knots or particles of duſt.

N° XVIII.

Account of a Worm in a Horſe's Eye, by F. HOPKINSON, *Eſquire.*

Read Sep. 26, 1783.

HAVING been myſelf a witneſs to the following curious fact, I thought it ſhould

not paſs unrecorded, eſpecially as it occurred in this city, under the immediate notice of the Philoſophical Society.

A report prevailed laſt ſummer that a horſe was to be ſeen which had a living ſerpent in one of his eyes. At firſt I diſregarded this report, but numbers of my acquaintance, who had been to ſee the horſe, confirming the account, I had the curioſity to go myſelf, taking a friend along with me. The horſe was kept in Arch-ſtreet and belonged to a free negroe. I examined the eye with all the attention in my power, being no ways diſpoſed to credit the common report, but rather expecting to detect a fraud or vulgar prejudice; I was much ſurpriſed, however, to ſee a real living worm within the ball of the horſe's eye. This worm was of a clear white colour, in ſize and appearance much like a piece of fine bobbin; it ſeemed to be from $2\frac{1}{2}$ to 3 inches in length, which however, could not be duly aſcertained, its whole length never appearing at one time, but only ſuch a portion as could be ſeen through the iris, which was greatly dilated. The creature was in a conſtant lively vermicular motion; ſometimes retiring ſo deep into the eye as to become totally inviſible, and at other times approaching ſo near to the iris as to become plainly and diſtinctly ſeen; at leaſt ſo much of it as was within the field of the iris. I could not diſtinguiſh its head, neither end being perfectly exhibited whilſt I viewed it, and indeed its motion was ſo briſk and conſtant, that ſo nice a ſcrutiny was not to be expected. The horſe's eye was exceedingly enflamed, ſwoln and running; I mean the muſcles contiguous to the eye ball, and ſeemed to give him great pain; ſo that it was with much difficulty the eye could be kept open for more than a few ſeconds at a time; and I was obliged to watch favourable moments for a diſtinct view of his tormentor. I believe the horſe was quite blind in that eye, for it appeared as if all the humours were confounded together, and that the worm had the whole orb to range in, which, however, was not

of

of a diameter ſufficient for the worm to extend its full length, as far as I could diſcover. The humours of the eye were beginning to grow opake like a chilled jelly, and became altogether ſo afterwards, as I was informed.

As this is a very uncommon circumſtance and may affect ſome philoſophical doctrines, it is much to be lamented that the horſe had not been purchaſed, and the eye diſſected for better examination. That there was a living, ſelf-moving worm within the ball of the horſe's eye, free from all deception or miſtake, I am moſt confident. How this worm got there, or if bred in ſo remarkable a place, where its parents came from, or how they contrived to depoſite their ſemen or convey their egg into the eye of an horſe, I leave for others to determine.

N° XIX.

An improved Method of Quilling a Harpſichord, by F. HOPKINSON, *Eſquire.*

Read Dec. 5, 1783.

MUCH of the pleaſure and effect in performing on a harpſichord depends on the equality of what is called *the touch;* and this is principally owing to a continuance of uniformity in the ſpring of the little quills, which by their impulſe ſet the ſtrings in vibration. Theſe quills, in the preſent manner of applying them, will not retain their elaſticity for any length of time, but require conſtant repair; which is one of the moſt troubleſome and difficult operations in keeping the inſtrument in order. To remedy this inconvenience, I have ſought for a ſubſtitute for the crow quill, and tried a variety of ſubſtances, but without ſucceſs. I then conſidered whether an improvement might not be made in the application of the quills themſelves, and to this purpoſe I examined.

examined the cauſe of the quills being ſo liable to break, and obſerved that the piece of quill is thruſt through a ſmall hole in the tongue of the jack, projecting only about a quarter of an inch beyond the face of the tongue: That this quill is too ſhort to yield in all its parts, and ſo act properly as a ſpring; but bends only at the place where it iſſues from the hole in the tongue, and works up and down as upon a hinge, in that place; and *there only* is the quill ever known to break.

Thus in Plate III, Figure 6, *a*, is the tongue, *b*, the quill fixed firmly in it, which being too ſhort to act fairly as a ſpring, will bend only at *c*, when it is forced to paſs the ſtring; and by repeated exerciſe muſt neceſſarily break in that part, as any ſpring would do if compelled to act in the ſame manner.

But if this quill could be made longer, or applied ſo that its ſpring ſhould be part of a curve, it would probably preſerve its elaſticity for any length of time, as other ſprings do.

To effect this I have conſtructed the tongue and applied the quill as repreſented in figure 7, where *a*, is the tongue, the top of which is rounded off; the quill is firmly fixed in the hole at *c*, as uſual, but inſtead of paſſing through a length ſufficient to ſtrike the ſtring, it is cut off even with the face of the tongue at *f*. The quill thus fixed with its poliſhed face downwards, is bent upward round the top of the tongue, and then proceeds horizontally the proper length; being kept in the horizontal poſition by the little wire ſtaple *e*, being firmly driven into holes drilled for the purpoſe, but not ſo far as to pinch the quill againſt the top of the tongue; a little ſpace being left for the quill to play in.

From this conſtruction it is manifeſt, that the ſpring of the quill will be in its whole length, but chiefly in the curve *c*, *d*; and that a quill ſo applied will act fairly as a ſpring, and may be expected to retain its elaſticity for years,

years, subject to no variations but such as may be occasioned by alterations in the state of the air, to which all known substances are more or less liable.

Read, 1784. IN the beginning of last winter, I had the honour to lay before the society an improved method of quilling a HARPSICHORD. Wishing to bring my discovery to the test of full experiment and to the judgment of abler critics, I forwarded a description and a model of my improvement to a friend in London, requesting that it might be submitted to the examination of proper judges, and directing, in case it should be approved of, that an instrument made by one of the first artists and quilled according to my proposed method, should be sent to me. I have accordingly received an excellent double harpsichord, made by Messrs *Shudi* and *Broadwood* of London, and quilled according to my method; with this difference, I had rounded off the top of the tongue, and bending the quill over it, kept it in a horizontal position by means of a small wire staple; as will be more fully understood by referring to my former description. But Mr. *Broadwood* has left the tongue of its full length and usual form: But made the hole, in which the quill is commonly fixed tight, so large, that the quill has free room to play therein; and then fixing the quill below, has bent it round and brought it through this hole; which renders a staple unnecessary; the top of the tongue answering the same purpose. The principle on which the improvement depends is the same in both; but his is the best method of executing it.

He informs, however, that one inconvenience occurs viz. the quills being so forcibly bent in the curved part, are liable, in some instances, to spring back, and so become not only too short to reach the string it should strike, but the projection of the curve will be apt to touch the string behind it, when the stop is pushed back.

To

To explain this, let *a*, *b*, figure 8, repreſent the tongue, *c*, *d*, *e*, the quill, firmly fixed at *c*, then bent upwards and brought through a hole, which is large enough for the quill to play freely therein. But the curved part of the quill at *d*, being ſo forcibly bent, will in ſome inſtances ſpring back (as repreſented in the figure) not keeping cloſe to the back of the tongue, as it ſhould do: And as there is no waſte room, the curve *d*, will be apt to touch the ſtring behind it, when the ſtop is puſhed back, I acknowledge that this inconvenience occurs in ſome few inſtances in the inſtrument Mr. *Broadwood* has ſent me; but would obſerve that as it does not *always* happen, it is a fault in the execution and not in the principle. Yet, as it may be difficult to guard againſt it, I have conſidered how this evil may be effectually prevented.

Inſtead of punching the ſmall hole, in which the quill is to be fixed, ſtraight through the tongue, let it be punched ſlanting downwards; this will relieve the quill from that ſtrained poſition which cauſes it to ſpring back. According to the firſt mode of application the curve formed by the quill will be as at *a*, figure 9, in the ſecond as at *b*. I have conſtructed many tongues in this way, and found none of them liable to the inconvenience complained of, or ſhewing any tendency whatever to ſpring back; but to remove all jealouſy on this head, ſhould any remain, it will be eaſy to drive a ſmall wire ſtaple againſt the bottom of the curve behind, which muſt effectually retain it cloſe to the back of the tongue.

I mention this expedient of the wire ſtaple merely with a view of removing all doubt; but I do not think it neceſſary; the objection being perfectly remedied by the other method: To prove this, I have cut out the entire block between the two holes, in the manner of a mortiſe, and drove a pin acroſs the upper part of it. I then cauſed the quill to lie in this ſlanting mortiſe, and bending it round brought it over the pin; and I found it would remain

main perfectly at eaſe in its birth, although not pinched or reſtrained in any part; *a*, figure 10, repreſents the tongue in front, and *b*, the mortiſe, of which the ſlanting ſhape cannot be ſeen in this view; but will be better underſtood by obſerving the poſition of the quill in figure 11, where *a* is a profile of the ſame tongue, *b*, *c*, the quill lying in the mortiſe, and *d*, the pin over which the top of the quill paſſes.

I have need to apologize to the ſociety for directing ſo much of their attention, to an object which may appear to ſome to be of little importance. To the muſical tribe, however, this improvement will preſent itſelf in a different light. Many perſons who play very well on the harpſichord, are not able to keep the inſtrument in order: And to ſend for a perſon to repair the quills and tune the inſtrument as often as it ſhall be neceſſary, is not only troubleſome and expenſive, but ſuch aſſiſtance is not always to be had, eſpecially in the country. And for theſe reaſons many a good harpſichord or ſpinnet lies neglected and the ſcholar looſes the opportunity of practice. To ſuch perſons a method of quilling that ſhall ſeldom want repair is a *diſideratum* of no ſmall importance. And this, I flatter myſelf I have accompliſhed.

The difficulty of *quilling* being thus removed, I conſidered in what manner *tuning* might be made eaſy to the practitioner in muſic. Harpſichords are tuned by means of *fifths* and *thirds*; but ſuch is the muſical diviſion of the monochord as to make it neceſſary, that none of theſe *fifths* or *thirds* ſhould be perfect; an allowance muſt be made; and to do this with judgment, ſo that the chords may be good and the inſtrument be in tune, requires much attention and practice. Of the numbers that play, there will not be found one in an hundred that can tune a harpſichord. To render this taſk eaſy, I have procured *twelve tuning forks*, for the *twelve ſemitones* of the octave; theſe I had perfectly tuned; and as they will not be ſenſibly af-

fected by any change of weather, they remain as ſtandards. I take it for granted that any perſon at all accuſtomed to muſical ſounds can tell when one tone is *in uniſon* with another; and that a very little practice will enable him to tune one ſound *an octave* to another, theſe conchords are ſo manifeſt that they cannot eaſily be miſtaken. There is then nothing to be done but to tune the twelve ſtrings in uniſon with the twelve forks; this will fix the ſcale, or temperature for one octave, which is the whole difficulty; the reſt of the inſtrument is eaſily tuned by uniſons and octaves to the ſcale, ſo aſcertained*.

Having, I hope, fully accompliſhed the deſign I had in view when I turned my thoughts to this ſubject, I ſhall now take leave of it; and ſhall be highly gratified if I find others benefited by my attentions, although in a matter of no very ſerious import.

Nov. 1784.

Deſcription of a further Improvement in the HARPSICHORD.

Read January 28, 1786.

IN a former paper read before the ſociety, reſpecting an improved method of quilling a HARPSICHORD, I made ſome apology for troubling you with a ſubject not ſtrictly within the limits of your view as a philoſophical ſociety, and which might appear to ſome of ſmall importance. At the ſame time I took formal leave of a purſuit which had accidentally engaged my attention, and which I had obtruded upon your's. Notwithſtanding this, I find myſelf under a neceſſity of again requeſting your indulgence, whilſt I deſcribe a diſcovery I made in Auguſt laſt, of a ſtill further improvement to the ſame purpoſe.

Having

* My ſet of forks are tuned from the middle C ſharp to the C above, incluſive.

Having ſucceeded to the extent of my expectation in a more advantageous way of applying the crow quill in common uſe in a harpſichord, I thought to reſt content with that improvement; which had principally for its object the duration of the quill's elaſticity, and of courſe the duration of the equality of touch. But notwithſtanding the long eſtabliſhed prejudice in favour of the crow quill, and the prevailing opinion that no ſubſtance can ſupply its place to advantage, I think a candid critic will allow that one of the following poſitions is founded in fact, and the other in reaſon.

Firſt. Although the three ſtops of a harpſichord ſhould be quilled to the beſt advantage, the reſult of the whole will be an obſervable jingle or tinkling between the quills and wires, which depreciates the dignity and ſweetneſs of the inſtrument. The beſt harpſichords are ſo cenſurable for this imperfection, that the *Forte Piano*, which is free from it, ſtands a chance of rivalling that noble inſtrument, for this cauſe only; being far inferior in every other reſpect.

Second. Is it not reaſonable to ſuppoſe that ſo long a ſtring, ſo advantageouſly ſtretched over ſo large a box, ſhould yield a greater body of tone, than that which is produced by the impulſe of a quill? If the quill be made very ſtiff, this will render the touch diſagreeable and encreaſe the jingle, but not add to the *body* of tone. One reaſon why the quill does not draw a fuller tone from the ſtring, I ſuppoſe to be the ſmallneſs of its contact. The back of a quill is a portion of a circle, the extended ſtring is a right line, and a circle can touch a right line only in a point; the contact therefore muſt be ſo very ſmall, that mere ſtrength of impulſe is not ſufficient to put the ſtring into full vibration.

The method I am now to deſcribe of quilling, or rather *tonguing* a harpſichord, I have found by experiment, to draw forth the powers of the inſtrument to a ſurpriſing

effect, causing it to yield a full and pure body of tone, free from all jingle and very pleasant to the ear.

N. B. What hath hitherto been called the tongue of the jack, I shall denominate the *palate;* and the substitute I have made for the quill, I shall call the *tongue.* The propriety of this will appear in the description.

Plate III. Let A, figure 12, represent the palate in front, with a mortise cut through it for the tongue to work in. B, is the tongue, having two small holes drilled through it, one in the centre of its motion and the other at a little distance behind, for the reception of one end of a wire spring hereafter mentioned.

Figure 13, is the palate in profile, with the tongue properly mounted and moveable on the centre pin. This figure also shews how the palate must be hollowed in behind to expose the root of the tongue, and the small hole in it for the reception of one end of the wire spring.

Figure 14, is a back view of the palate, shewing the groove in which the hair spring of the jack lies, and a small wire staple at *b*, to which the lower end of the steel spring is to be fastened.

Figure 15, is the spring which is to govern the tongue. It must be of fine steel wire, somewhat annealed by being forcibly rubbed between pieces of leather or cork, and is formed by winding the wire backwards and forwards with a tight hand, over pins driven deep and firm into a piece of wood. As the palate must play freely within the fork or jaws of the jack, the windings of the spring must not exceed the width of the palate. The upper end of the spring being run through the small hole in the root of the tongue and bent round, so as to secure it, and the superfluous part cut off; the lower end of the same spring must be run under the little staple (*b*, figure 14,) and bent upwards with a gentle strain, so as to hook it on and secure it to that staple; the spring will then operate with all its elasticity, and the tongue will be subjected to its operation.

Figure

Figure 17, repreſents the palate in a back view with the zig zag ſpring faſtened by one end to the root of the tongue, and by the other to the little ſtaple.

To prevent the tongue from riſing by the force of the ſpring above a horizontal poſition, there muſt be a wire ſtaple driven in the front of the palate immediately above the tongue (as at *a*, in figure 12 and 13;) and the tongue, if of wood, ſhould be armed with a ſmall piece of ſoft leather juſt under the ſtaple, to prevent noiſe.

It muſt be left to future experiment to determine the moſt proper of all ſubſtances of which the tongue ſhould be made; different ſubſtances drawing different tones from the ſtring. After many eſſays to this purpoſe, I have concluded to furniſh my harpſichord in the following manner.

The tongues of the firſt uniſon are of *Ben ſole-leather*. Thoſe of the ſecond are of a ſoft leather faced with Morocco, ſuch as is frequently uſed in harpſichords, though applied in a different way, and the tongues of the octave are of wood, ſuch as pear tree, laurel, or any wood of an even grain and not too hard in ſubſtance. But all mounted on ſprings, as above deſcribed, and their faces well poliſhed with black lead where they come in contact with the ſtrings.

My reaſons are. The ſole-leather produces a full, ſweet and vigorous tone from the firſt uniſon. The ſecond uniſon, which is the piano of the inſtrument when the pedal is preſſed, is furniſhed with Morocco leather, which draws a full but more ſoft and ſmothered tone from the ſtring. And the octave is ſtruck with wooden tongues for the ſake of vivacity or brilliancy, which is the genius of that ſtop; yet I am not ſure but that the octave alſo had better be ſtruck with ſole-leather, like the firſt uniſon*.

A harpſichord thus furniſhed, will produce a body or quantity of ſound, and a purity of tone, that will aſtoniſh at

* Becauſe, after the ſtroke has been given, the wooden tongue repaſſing the ſtring, yet in vibration, makes a jingle, which the leather tongues do not.

at the firſt hearing, much reſembling the diapaſon ſtop of an organ. And it is manifeſt that if the touch be well regulated at firſt, it will not afterwards be ſubject to alteration for a long courſe of time. The touch is in part regulated by the ſtrength of the ſerpentine ſpring and the number of its zig zag evolutions; and in part by the manner of rounding off the tip of the tongue; for the tip of the tongue muſt not be cut off ſquare, (in which caſe, the ſtring would leave the tongue too abruptly and cauſe a diſagreeable twang,) but ſhould be ſlanted off from underneath, and its extreme point rounded and well poliſhed by rubbing it very hard with a piece of black lead. As to the ſtrength of the ſpring, four ſizes of wire, viz. from n°. 4 to n°. 8, will be ſufficient for the whole inſtrument; but the touch is more immediately regulated by rounding off the tips of the tongues by the preſſure and poliſh of the black lead, more or leſs, as occaſion ſhall require. When the tongues are of wood, a ſtroke or two of a fine file will be neceſſary to take off the ſquare edge left by the knife, previous to the poliſhing it with the black lead.

After all, a harpſichord juſt furniſhed in this way, will not be ſo pleaſant to the touch or to the ear as it will be after a few weeks uſe; when the ſtrings will, by repeated friction, have rounded off and poliſhed the tips of the tongues, and have made for themſelves a broad bearing or contact, which cannot perhaps be ſo accurately produced by any care of the workman.

Laſtly, it is ſcarce neceſſary to obſerve that the ſerpentine ſpring and the root of the tongue muſt be compriſed within the thickneſs of the jack; otherwiſe they will be apt to interfere with the ſtring behind, when the ſtop is puſhed back.

F. HOPKINSON.

Obſervations

N° XX.

Obſervations on a Comet lately diſcovered; communicated by DAVID RITTENHOUSE, *Eſquire.*

Read Mar. 19, 1784. ON the 21ſt of January laſt, John Lukens, Eſquire, informed me that he had diſcovered a comet the preceding evening, and on the evening of the ſame day, aſſiſted by Mr. Lukens and Mr. Prior, I obſerved the apparent place of the comet to be in the 15th degree of Piſces, with 16° 6′ ſouth latitude. By ſubſequent obſervations I found its motion to be north eaſterly, with reſpect to the ecliptic, and that its neareſt approach to us had preceded our firſt obſervation. It paſſed the ecliptic on the 31ſt in the 25° of Piſces, and February the 17th it was in Piſces 29° with 13° 10′ north latitude. This was the laſt time I ſaw it, clouds and moonlight having ſince prevented.

The light of this comet was ſo very faint that it was impoſſible to obſerve it with accuracy, at leaſt without better inſtruments than I am poſſeſſed of, eſpecially as the comet was always involved in day light, moonlight or the thick atmoſphere of the horizon. No pains or attention however were wanting, and from the beſt obſervations I could make, I find it paſſed its perihelion about the 20th of January, its diſtance from the ſun being about $\frac{7}{10}$ of the ſun's diſtance from us. The place of its aſcending node is in the 25th deg. of Taurus, and the inclination of its orbit 53°. Its motion is retrograde, that is, contrary to the order of the ſigns. I have ſtill hopes of ſeeing it in the morning, though its diſtance is now ſo very great that it can ſcarcely be viſible to the naked eye.

Extract

N° XXI.

Extract of a Letter from the Rev. JEREMY BELKNAP, *containing Obſervations on the Aurora Borealis.*

Dover, New-Hampſhire, March 31ſt, 1783.

Read May 2, 1783. "DID you ever, in obſerving the Aurora Borealis, perceive a *ſound?* I own I once looked on the idea as frivolous and chimerical, having heard it at firſt from perſons whoſe credulity, I ſuppoſed, exceeded their judgment; but, upon hearing it repeatedly, and from ſome others whom I thought judicious and curious, I began to entertain an opinion in favour of it. I was ſtrengthened in this opinion about two years ago, by liſtening with attention to the flaſhing of a luminous arch which appeared in a calm froſty night, when I thought I heard a faint ruſtling noiſe like the bruſhing of ſilk. Laſt Saturday evening I had full auricular demonſtration of the reality of this phenomenon. About ten o'clock the hemiſphere was all in a glow; the vapours aſcended from all points, and met in a central one in the zenith: All the difference between the ſouth and north part of the heavens was, that the vapour did not begin to aſcend ſo near the horizon in the ſouth as in the north. There had been a ſmall ſhower with a few thunder claps, and a bright rainbow in the afternoon; and there was a gentle weſtern breeze in the evening which came in flaws, with intervals of two or three minutes; in theſe intervals I could plainly perceive the ruſtling noiſe, which was eaſily diſtinguiſhable from the ſound of the wind, and could not be heard till the flaw had ſubſided. The flaſhing of the vapour was extremely quick; whether accelerated by the wind I cannot

cannot ſay; but from that quarter where the greateſt quantity of the vapour ſeemed to be in motion, the ſound was plaineſt; and this, during my obſervation, was the eaſtern. The ſcene laſted about half an hour, though the whole night was as light as when the moon is in the quarters."

N° XXII.

A Letter from J. MADISON, *Eſq. to* D. RITTENHOUSE, *Eſq. containing Experiments and Obſervations upon what are commonly called the Sweet Springs.*

THESE waters riſe on the north ſide of a large mountain at the foot of it, called the Sweet Spring Mountain, in the county of Botetourt. The ſouth ſide is covered with ſtones of an ocrous appearance. In many places iron ore may be found; but on the north the mountain is fertile, covered with a rich mould, at leaſt near the ſpring. The remarkable efficacy of theſe waters in many diſorders, eſpecially, it is ſaid, in conſumptive complaints, firſt induced me to attempt their analyſis. Such experiments as I had time and opportunity to make, I ſhall faithfully relate, and leave it to others, better qualified than myſelf, to judge of their merits.

Experiment 1. Having plunged a very ſenſible mercurial thermometer in the ſpring, it ſtood at 73°. The temperature of air was about 69.

2. A good hydrometer ſunk one-twentieth of an inch deeper in common mountain water, than in the ſpring.

3. Nut-galls mixed with the water in a wine glaſs ſtruck a paliſh brown, which ſhewed that there was little or no iron in it.

4. Violets mixed with the water in a wine glaſs, turn-

ed it in a ſhort time of a reddiſh colour. This was a proof that the waters contained ſome kind of acid.

5. Having made a ſolution of ſilver in the nitrous acid, and mixed a little of it with the water, it immediately became milky, and a white pulvurent precipitate enſued. This experiment ſhewed by the whiteneſs of the precipitate, that the waters contained nothing ſulphureous, and by the pulvurency of the precipitate that the acid contained in the waters was vitriolic.

6. A ſolution of lead in the nitrous acid being mixed with the water, it became ſomewhat milky, and a white precipitate was obſerved. This experiment alſo ſhews that the waters contain an acid, moſt probably the vitriolic, and alſo that they contain calcareous earth. Soap is not readily miſcible with them.

7. A ſolution of ſaccharum ſaturni in the nitrous acid being made, and lines marked upon paper with it, and placed over the water, the lines retained their former colour. This experiment alſo ſhews that the water contains nothing ſulphureous.

8. Having poured a little of the ſpirit of ſalt into the water, after ſome time a coloured precipitate was obſerved, but as the waters did not ſtrike a green or blue colour, it ſhewed that there was no copper in them.

9. A ſolution of vitriol of copper mixed with the water produced a thick, green, curdly appearance, but did not become bluer. This experiment ſhewed that there was no vol. alkali contained in them.

10. The vitriolic acid mixed with the water ſuddenly effervеſced, and produced a heat which raiſed the thermometer from 75 to 83, by applying the bulb to the outſide of the glaſs.

11. As the ſpring is continually diſcharging large bubbles of air, which riſing from the bottom break upon the ſurface of the water, I was deſirous of making ſome experiments upon the air, in order to determine whether the

the acidity of the water might not be owing to it; and alſo to determine the nature of the air, whether fixed or not. Having therefore caught a quantity of the air in a decanter, I communicated a part of it to an equal bulk of pure mountain water, and after agitating them for ſome time, gave it to ſeveral to taſte; who agreed that it had the taſte of the ſpring water. Upon a ſecond trial this experiment did not ſucceed. I had not an opportunity of trying the nature of the air by means of chalk-water, and was prevented from proſecuting any farther enquiries into the nature of theſe celebrated waters by a ſudden alarm, to which the frontiers were then continually expoſed.

Theſe waters have been falſely called *ſweet*, for their taſte is evidently acidulous. The experiments alſo ſhew that they contain an acid. Their taſte reſembles exactly that of waters artificially impregnated with fixed air, extricated from chalk, by means of the vitriolic acid, and I conceive muſt be nearly the ſame with the true Pyrmont water. They have little or no ſmell, do not form an incruſtation, nor do they leave a depoſit upon ſtanding many hours. Upon bathing in the morning, the ſkin has a ſoapy kind of feel. This was not obſerved in the evening.

There is near this ſpring another, a very ſtrong chalybeate.

I am, with great regard, yours,

J. MADISON.

N° XXIII.

A Letter from the Rev. JEREMY BELKNAP, *on the preſerving of Parſnips by drying.*

Dover, New-Hampſhire, March 5, 1784.

SIR,

Read Apr. 16, 1784.

AMONG the number of eſculent roots, the *parſnip* has two ſingular good qualities.

One is that it will endure the ſevereſt froſt and may be taken out of the ground in the ſpring, as freſh and ſweet as in autum; the other is that it may be preſerved by drying to any deſired length of time.

The firſt of theſe advantages has been known for many years paſt; the people in the moſt northerly parts of New-England where winter reigns with great ſeverity, and the ground is often frozen to the depth of two or three feet for four months, leave their parſnips in the ground till it thaws in the ſpring, and think them much better preſerved than in cellars.

The other advantage never occurred to me till this winter, when one of my neighbours put into my hands a ſubſtance which had the appearance of a piece of buck's horn. This was part of a parſnip which had been drawn out of the ground laſt April and had lain neglected in a dry cloſet for ten months. It was ſo hard as to require conſiderable ſtrength to force a knife through it croſs-wiſe; but being ſoaked in warm water, for about an hour, became tender, and was as ſweet to the taſte as if it had been freſh drawn from the ground.

As many uſeful diſcoveries owe their origin to accident, this may ſuggeſt a method of preſerving ſo pleaſant and wholeſome a vegetable for the uſe of ſeamen in long voyages, to prevent the ſcurvey and other diſorders incident to a ſea-faring life, which is often rendered tedious and diſtreſſing for want of vegetable food; ſince I am perſuaded that parſnips dried to ſuch a degree, as above related, and packed in tight caſks, may be tranſported round the globe, without any loſs of their flavour or diminution of their nutritive quality.

I am ſir, your humble ſervant,

JEREMY BELKNAP.

An

N° XXIV.

An Optical Problem, propoſed by Mr. HOPKINSON, *and ſolved by* Mr. RITTENHOUSE.

Philadelphia, March 16th, 1785.

DEAR SIR,

Read Feb. 17, 1786. I TAKE the liberty of requeſting your attention to the following problem in *optics*. It is I believe entirely new, and the ſolution will afford amuſement to you and inſtruction to me.

Setting at my door one evening laſt ſummer, I took a ſilk handkerchief out of my pocket, and ſtretching a portion of it tight between my two hands, I held it up before my face and viewed, through the handkerchief, one of the ſtreet lamps which was about one hundred yards diſtant; expecting to ſee the threads of the handkerchief much magnified. Agreeably to my expectation I obſerved the ſilk threads magnified to the ſize of very coarſe wires; but was much ſurpriſed to find that, although I moved the handkerchief to the right and left before my eyes, the dark bars did not ſeem to move at all, but remained permanent before the eye. If the dark bars were occaſioned by the interpoſition of the magnified threads between the eye and the flame of the lamp, I ſhould have ſuppoſed that they would move and ſucceed each other, as the threads were made to move and paſs in ſucceſſion before the eye; but the fact was otherwiſe.

To account for this phenomenon exceeds my ſkill in *optics*. You will be ſo good as to try the experiment, and if you find the caſe truly ſtated, as I doubt not you will, I ſhall be much obliged by a ſolution on philoſophical principles. I am ſir, with great ſincerity,

Your moſt effectionate friend,
And very humble ſervant,
F. HOPKINSON.

The Anſwer, by Mr. RITTENHOUSE.

DEAR SIR,

THE experiment you mention, with a ſilk handkerchief and the diſtant flame of a lamp, is much more curious than one would at firſt imagine. For the object we ſee is not the web of the handkerchief magnified, but ſomething very different, as appears from the following conſiderations. 1ſt. A diſtinct image of any object, placed cloſe to the eye, cannot be formed by parallel rays, or ſuch as iſſue from a diſtant luminous point: for all ſuch rays, paſſing through the pupil, will be collected at the bottom of the eye, and there form an image of the luminous point. The threads of the handkerchief would only intercept part of the rays, and render the image leſs brilliant. 2dly. If the croſs bars we ſee were images of the ſilk threads, they muſt paſs over the retina, whilſt the threads are made to paſs over the pupil; but this, as you obſerve, does not happen; for they continue ſtationary. 3dly. If the image on the retina was a picture of the object before the eye, it muſt be fine or coarſe, according to the texture of the handkerchief. But it does not change with changing the ſilk, nor does it change on removing it farther from the eye. And the number of apparent threads remains the ſame, whether 10, 20, or 30 of the ſilk threads paſs acroſs the pupil at the ſame time. The image we ſee muſt therefore be formed in ſome different manner; and this can be no other than by means of the *inflection* of light in paſſing near the ſurfaces of bodies, as deſcribed by NEWTON.

It is well known in optics that different images of the different points of objects without the eye are formed on the retina by pencils of rays, which, before they fall on the eye, are inclined to each other in ſenſible angles. And the great uſe of teleſcopes is to encreaſe theſe angles, regularly, in a certain ratio; ſuffering ſuch rays as were parallel

parallel before they enter the teleſcope to proceed on, parallel, after paſſing through it. The extended image which we ſee in this experiment muſt therefore be formed by pencils of rays, which before they entered the eye, had very conſiderable degrees of inclination with reſpect to each other. But coming from a ſmall diſtant flame of a lamp, they were nearly parallel before they paſſed through the ſilk handkerchief. It was therefore the threads of ſilk which gave them ſuch different directions.

Before the ſilk is placed to the eye, parallel rays of light will form a ſingle lucid ſpot, as at A, Plate III. Figure 16. And this ſpot will ſtill be formed afterwards by ſuch rays as paſs through the little meſhes uninfluenced by the threads. But ſuppoſe the perpendicular threads by their action on the rays, to bend a part of them one degree to the right and left, another part two degrees; there will now be four new images formed, two on each ſide of the original one at A. By a ſimilar action of the horizontal threads, this line of five lucid points will be divided into five other lines, two above and two below, making a ſquare of twenty-five bright ſpots, ſeparated by four perpendicular dark lines and four horizontal ones; and theſe lucid ſpots and dark lines will not change their places on moving the web of ſilk over the eye parallel to any of its threads. For the point of the retina on which the image ſhall fall is determined by the incidence of the rays, with reſpect to the axis of the eye, before they enter, and not by the part of the pupil through which they paſs.

In order to make my experiments with more accuracy, I made a ſquare of parallel hairs about half an inch each way. And to have them nearly parallel and equidiſtant, I got a watchmaker to cut a very fine ſcrew on two pieces of ſmall braſs wire. In the threads of theſe ſcrews, 106 of which made one inch, the hairs were laid 50 or 60 in number. Looking through theſe hairs at a ſmall opening in the window ſhutter of a dark room, $\frac{1}{30}$ of an inch wide and

and three inches long, holding the hairs parallel to the ſlit, and looking toward the ſky, I ſaw three parallel lines, almoſt equal in brightneſs, and on each ſide four or five others much fainter and growing more faint, coloured and indiſtinct, the farther they were from the middle line, which I knew to be formed by ſuch rays as paſs between the hairs uninfluenced by them. Thinking my apparatus not ſo perfect as it might be, I took out the hairs and put in others, ſomething thicker, of theſe 190 made one inch, and therefore the ſpaces between them were about the $\frac{1}{250}$ part of an inch. The three middle lines of light were now not ſo bright as they had been before, but the others were ſtronger and more diſtinct, and I could count ſix on each ſide of the middle line, ſeeming to be equally diſtant from each other, eſtimating the diſtance from the centre of one to the centre of the next. The middle line was ſtill well defined and colourleſs, the next two were likewiſe pretty well defined, but ſomething broader, having their inner edges tinged with blue and their outer edges with red. The others were more indiſtinct, and conſiſted each of the priſmatic colours, in the ſame order, which by ſpreading more and more, ſeemed to touch each other at the fifth or ſixth line, but thoſe neareſt the middle were ſeparated from each other by very dark lines, much broader than the bright lines.

Finding the beam of light which came through the window ſhutter divided into ſo many diſtinct pencils, I was deſirous of knowing the angles which they made with each other. For this purpoſe I made uſe of a ſmall priſmatic teleſcope and micrometer, with which I was favoured by Dr. Franklin. I faſtened the frame of parallel hairs before the object glaſs, ſo as to cover its aperture entirely. Then looking through the teleſcope, I meaſured the ſpace between the two firſt ſide lines, and found the angular diſtance between their inner edges to be $13'$, $15''$; from the middle of one to the middle of the other $15'$, $30''$, and from

from the outer edge of one, to the outer edge of the other $17'$, $45''$. In the firſt caſe I had a fine blue ſtreak in the middle of the object, and in the laſt a red ſtreak. The other lines were too faint, when ſeen through the teleſcope, to meaſure the angles they ſubtended with accuracy, but from ſuch trials as I made I am ſatisfied that from the ſecond line on one ſide to the ſecond on the other ſide, and ſo on, they were double, triple, quadruple, &c. of the firſt angles.

It appears then that a very conſiderable portion of the beam of light paſſed between the hairs, without being at all bent out of its firſt courſe; that another ſmaller portion was bent at a medium about $7'$, $45''$ each way; the red rays a little more, and the blue rays a little leſs; another ſtill ſmaller portion $15'$, $30''$; another $23'$, $15''$, and ſo on. But that no light, or next to none, was bent in any angle leſs than $6'$, nor any light of any particular colour, in any intermediate angle between thoſe which ariſe from doubling, tripling, &c. of the angle in which it is bent in the firſt ſide lines.

I was ſurprized to find that the red rays are more bent out of their firſt direction, and the blue rays leſs; as if the hairs acted with more force on the red than on the blue rays, contrary to what happens by refraction, when light paſſes obliquely through the common ſurface of two different mediums. It is, however, conſonant to what Sir Iſaac Newton obſerves with reſpect to the fringes that border the ſhadows of hairs and other bodies; his words are, "And therefore the hair in cauſing theſe fringes, "acted alike upon the red light or leaſt refrangible rays "at a greater diſtance, and upon the violet or moſt re-"frangible rays at a leſs diſtance, and by thoſe actions "diſpoſed the red light into larger fringes, and the violet "into ſmaller fringes."

By purſuing theſe experiments it is probable that new and intereſting diſcoveries may be made, reſpecting the

properties of this wonderful ſubſtance, light, which animates all nature in the eyes of man, and perhaps above all things diſpoſes him to acknowledge the Creator's bounty. But want of leiſure obliges me to quit the ſubject for the preſent.

I am, dear ſir, your affectionate friend,
And very humble ſervant,
DAVID RITTENHOUSE.

N° XXV.

An Enquiry into the Cauſe of the Increaſe of Bilious and Intermitting Fevers in Pennſylvania, with Hints for preventing them. By BENJAMIN RUSH, M. D. *Profeſſor of Chemiſtry in the Univerſity of Pennſylvania.*

Read December 16, 1785.

IT has been remarked, that Pennſylvania for ſome years paſt has become more ſickly than formerly. Fevers which a few years ago appeared chiefly on the banks of creeks and rivers, and in the neighbourhood of mill-ponds, now appear in parts remote from them all, and in the higheſt ſituations. This change with reſpect to the healthineſs of our country, may be traced to the three following cauſes.

1. The eſtabliſhment and increaſe of mill-ponds. There are whole counties in Pennſylvania in which intermittents were unknown, until the waters in them were dammed, for the purpoſe of erecting mill-ponds.

2. The cutting down of wood, under certain circumſtances, tends to render a country ſickly. It has been remarked that intermittents on the ſhores of the Suſquehannah have kept an exact pace with the paſſages which have been opened for the propagation of marſh effluvia, by cutting

cutting down the wood which formerly grew in its neighbourhood. I remember the time, when intermittents were known only within half a mile, in ſome places, of that river. They are now to be met with ten miles from it in the ſame parts of the ſtate.

I beg a diſtinction to be made here between *clearing* and *cultivating* a country. While clearing a country makes it ſickly in the manner that has been mentioned, *cultivating* a country, that is, draining ſwamps, deſtroying weeds, burning bruſh, and exhaling the unwholſome or ſuperfluous moiſture of the earth, by means of frequent crops of grain, graſſes, and vegetables of all kinds, render it healthy. I could mention, in ſupport of theſe facts, ſeveral countries in the United States, which have paſſed through each of the ſtages that have been deſcribed. The firſt ſettlers received theſe countries from the hands of nature pure and healthy*. Fevers ſoon followed their improvements, nor were they finally baniſhed, until the higher degrees of cultivation that have been named took place. I confine myſelf to thoſe countries only where the ſalutary effects of cultivation were not rendered abortive by the neighbourhood of mill-ponds.

A 3d cauſe of the late increaſe of bilious and intermitting fevers, muſt be ſought for in the different and unequal quantities of rain which have fallen within theſe laſt ſeven years. While our creeks and rivers, from the uniformity of our ſeaſons, were confined to ſteady bounds, there was little or no exhalation of febrile miaſmata from their ſhores. But the dry ſummers of 1780, 1781, and 1782, by reducing our creeks and rivers far below their ancient marks; while the wet ſprings of 1784 and 1785, by ſwelling them both beyond their natural heights, have, when they have fallen, as in the former caſe, left a large

* A phyſician who travelled through part of Bedford county, in Pennſylvania, in the year 1782, informed me that he was witneſs of ſome country people having travelled twenty miles, to ſee whether it was poſſible for a German girl who laboured under an intermittent, to be HOT and COLD at the ſame time.

and extenſive ſurface of moiſt ground expoſed to the action of the ſun, and of courſe to the generation and exhalation of febrile miaſmata. The hiſtory of epidemics in foreign countries, favours this opinion of the cauſe of their increaſe in Pennſylvania. The inhabitants of Egypt are always healthy during the overflowing of the Nile. Their fevers appear only after the receſs of the river. It is remarkable that a wet ſeaſon is often healthy in low, while it is ſickly in hilly countries. The reaſon is obvious. In the former the rains entirely cover all the moiſt grounds, while in the latter, they fall only in a ſufficient quantity to produce thoſe degrees of moiſture which favour febrile exhalations. The rains which fall in the ſummer are rendered harmleſs only by covering the *whole* ſurface of marſhy ground. The rains which fall in our ſtate after the middle of September, are ſo far from producing fevers, that they generally prevent them. The extraordinary healthineſs of the laſt autumn, I believe was occaſioned by nothing but the extraordinary quantity of rain that fell during the autumnal months. The rain probably acts at this ſeaſon by diluting, and thus deſtroying, the febrile miaſmata that were produced by the heat and moiſture of the preceding ſummer. In ſupport of the truth of this third cauſe of the increaſe of fevers in Pennſylvania, I have only to add a fact lately communicated to me by Dr. Franklin. He informed me that in his journey from Paſſy to Havre de Grace, laſt ſummer, he found the country through which he travelled, unuſually ſickly with fevers. Theſe fevers it was generally ſuppoſed, were produced by the extraordinary dry weather, of which the public papers have given us ſuch melancholy and frequent accounts.

I come now to ſuggeſt a few hints for obviating and preventing fevers, and for rendering our country again healthy. For this purpoſe I beg leave to recommend in the firſt place, the planting of trees around all our mill-ponds, (beſides cleaning them occaſionally) in order to prevent

prevent the diſeaſes that have juſtly been aſcribed to them. Let the trees be planted in the greateſt number, and cloſeſt together, to leeward of the ordinary current of the ſummer and autumnal winds. I have known ſeveral inſtances of families being preſerved from fevers by an accidental copſe of wood ſtanding between a mill-pond and a dwelling houſe, and that in caſes too where the houſe derived no advantage from an high ſituation. The trees *around* or *near* a mill-pond, act perhaps in a ſmall degree *mechanically*. By ſheltering the pond from the action of the ſun, they leſſen exhalation, as well as obſtruct the paſſage of the vapors that are raiſed to the adjacent parts. But they act likewiſe *chemically*. It has been demonſtrated that trees abſorb unhealthy air, and diſcharge it in a highly purified ſtate in the form of what is now called "de-"phlogiſticated" air. The willow tree, according to Mr. Ingenhauſz, has been found to purify air the moſt rapidly of any tree that he ſubjected to his experiments. The rapidity of its growth, its early verdure, and the late fall of its leaf, all ſeem to mark it likewiſe as a tree highly proper for this purpoſe.

A ſecond method of preventing fevers, is to let the cultivation always keep pace with the *clearing* of our lands. Nature has in this inſtance connected our duty, intereſt and health together. Let every ſpot covered with moiſture from which the wood has been cut, be carefully drained, and afterwards ploughed and ſowed with graſs ſeed; let weeds of all kinds be deſtroyed, and let the waters be ſo directed as to prevent their ſtagnating in any part of their courſe.

Theſe are the two principal means of extirpating intermitting and bilious fevers from our country, but as theſe means are ſlow in their operation, I ſhall ſubjoin a few directions for preventing fevers till the above remedies can take effect.

1. Whether

1. Whether the matter which produces fevers be of an organic, or inorganic nature, I do not pretend to determine, but it is certain, that *fire* or the *ſmoke* or *heat*, which iſſue from it, deſtroy the effects of marſh miaſmata upon the human body; hence we find cities more healthy than country places, and the centre of cities more healthy than their ſuburbs in the ſickly months. To derive the utmoſt poſſible benefit from this method of preventing ſickneſs, I would adviſe large fires to be made every evening of bruſh between the ſpots from whence the exhalations are derived, and the dwelling houſe, and as near to the latter as is ſafe, and not diſagreeable. This practice ſhould be continued till the appearance of two or three froſts, for froſts as well as heavy rains in the autumnal months never fail to put a ſtop to the progreſs of intermittents.

During the ſickly ſeaſon, fires ſhould be likewiſe kept in every room in the dwelling houſe, even in thoſe caſes where the heat of the weather makes it neceſſary to keep the doors and windows open.

2. Let me adviſe my countrymen in ſickly ſituations, to prefer woolen and cotton to linen clothes in the ſummer and autumnal months. The moſt ſickly parts of the iſland of Jamacia have been rendered more healthy, ſince the inhabitants have adopted the uſe of woolen and cotton garments inſtead of linen.

During the late war, I knew many officers both in the Britiſh and American armies who eſcaped fevers in the moſt ſickly places, by wearing woolen ſhirts, or waiſtcoats conſtantly next to their ſkins. I have heard the preſent diminution of the human body in ſtrength and ſize, compared with its ancient vigor and form, aſcribed in part to the introduction of linen garments. I am not diſpoſed to controvert this opinion, but I am ſure of the efficacy of woolen clothes in wet and cold climates in preventing fevers of all kinds. The parliament of Great Britain

Britain compels every body that dies within the iſland to be buried in a woolen ſhirt or winding ſheet. The law would be much wiſer if it compelled every body to wear woolen garments next to their ſkins during life, and linen after death.

3. The diet in the ſickly months ſhould be generous. Wine and beer ſhould be the drinks of this ſeaſon inſtead of ſpirits and water. I do not think that fruit and vegetables of any kind produce fevers, but as the ſeaſon of the year produces languor and weakneſs, a larger quantity of animal food than uſual is beſt calculated to oppoſe them. Salted meat for this reaſon is preferable to freſh meat. Food of all kinds eaten during the ſickly months ſhould be well ſeaſoned.

4. The evening air ſhould be avoided as much as poſſible. There are at preſent few places in Pennſylvania where it is ſafe to ſleep, or even to ſet, after the going down of the ſun, in the ſickly months, with the windows open. The morning air before the ſun riſes, ſhould not be breathed, until the body has been fortified with a little ſolid aliment, or a *draught* of bitters. Theſe bitters ſhould be made of centaury, wormwood, camomile, or the bark of the willow or dogwood trees, infuſed in *water*. Bitters made with ſpirits, or even wine, cannot be taken in a ſufficient quantity to do ſervice, without producing intoxication, or the deadly habit of loving and drinking ſpirituous liquors.

5. Too much cannot be ſaid in favour of cleanlineſs, as a means of preventing fevers. The body ſhould be bathed or waſhed frequently. It has been proved that in the highlands of Jamaica adding ſalt to water, renders it more powerful in preventing diſeaſes when applied to the body. Equal pains ſhould be taken to promote cleanlineſs in every ſpecies of apparel. Offal matters, eſpecially thoſe which are of a vegetable nature, ſhould be removed from the neighbourhood of a dwelling houſe. The dung of domeſtic animals during its progreſs towards manure

may

may be excepted from this direction. Nature, which made man and theſe animals, equally neceſſary to each other's ſubſiſtence, has kindly prevented any inconvenience from their living together. On the contrary, to repay the huſbandman for affording a ſhelter to theſe uſeful and helpleſs animals, nature has done more. She has endowed their dung with a power of deſtroying the effects of marſh exhalations, and of preventing fevers. The miſerable cottagers in Europe who live under the ſame roof, and in ſome inſtances in the ſame room with their cattle, are always healthy. In Philadelphia, fevers are leſs known in the neighbourhood of livery ſtables, than in any other part of the city. I could mention a family that has lived near thirty years near a livery ſtable in a ſickly part of the city, that has never known a fever but from the meaſles or ſmall-pox.

N° XXVI.

An Account of the late Dr. HUGH MARTIN's *Cancer Powder, with brief Obſervations on Cancers. By* BENJAMIN RUSH, M. D. *&c. &c.*

Read February 3, 1786.

A FEW years ago a certain Dr. Hugh Martin, a ſurgeon of one of the Pennſylvania regiments ſtationed at fort Pitt, during the latter part of the late war, came to this city, and advertiſed to cure cancers with a medicine which he ſaid he had diſcovered in the woods, in the neighbourhood of the garriſon. As Dr. Martin had once been a pupil of mine, I took the liberty of waiting upon him, and aſked him ſome queſtions reſpecting his diſcovery. His anſwers were calculated to make me believe, that his medicine was of a vegetable nature, and that it was originally an Indian remedy. He ſhewed

ſhewed me ſome of the medicine, which appeared to be the powder of a well dried root of ſome kind. Anxious to ſee the ſucceſs of this medicine in cancerous ſores, I prevailed upon the doctor to admit me to ſee him apply it in two or three caſes. I obſerved in ſome inſtances, he applied a powder to the parts affected, and in others only touched them with a feather dipped in a liquid which had a white ſediment, and which he made me believe was the vegetable root diffuſed in water. It gave me great pleaſure to witneſs the efficacy of the doctor's applications. In ſeveral cancerous ulcers, the cures he performed were complete. Where the cancers were much connected with the lymphatic ſyſtem, or accompanied with a ſcrophulous habit of body, his medicine always failed, and in ſome inſtances did evident miſchief.

Anxious to diſcover a medicine that promiſed relief in even a few caſes of cancers, and ſuppoſing that all the cauſtic vegetables were nearly alike, I applied the phytolacca or poke root, the ſtramonium, the arum, and one or two others, to foul ulcers, in hopes of ſeeing the ſame effects from them which I had ſeen from Dr. Martin's powder, but in theſe I was diſappointed. They gave ſome pain, but performed no cures. At length I was furniſhed by a gentleman from fort Pitt with a powder which I had no doubt, from a variety of circumſtances, was of the ſame kind as that uſed by Dr. Martin. I applied it to a fungous ulcer, but without producing the degrees of pain, inflammation, or diſcharge, which I had been accuſtomed to ſee from the application of Dr. Martin's powder. After this, I ſhould have ſuſpected that the powder was not a *ſimple* root, had not the doctor continued upon all occaſions to aſſure me that it was wholly a vegetable preparation.

In the beginning of the year 1784 the doctor died, and it was generally believed that his medicine had died with him. A few weeks after his death, I procured from Mr. Thomas Lieper, one of his adminiſtrators, a few ounces of

the doctor's powder, partly with a view of applying it to a cancerous ſore which then offered, and partly with a view of examining it more minutely than I had been able to do during the doctor's life. Upon throwing the powder, which was of a brown colour, upon a piece of white paper, I perceived diſtinctly a number of white particles ſcattered through it. I ſuſpected at firſt that they were corroſive ſublimate, but the uſual teſts of that metallic ſalt ſoon convinced me that I was miſtaken. Recollecting that arſenic was the baſis of moſt of the celebrated cancer powders that have been uſed in the world, I had recourſe to the teſts for detecting it. Upon ſprinkling a ſmall quantity of the powder upon ſome coals of fire, it emitted the garlic ſmell ſo perceptibly as to be known by ſeveral perſons whom I called into the room where I made the experiment, and who knew nothing of the object of my enquiries. After this with ſome difficulty I picked out about three or four grains of the white powder, and bound them between two pieces of copper, which I threw into the fire. After the copper pieces became red hot, I took them out of the fire, and when they had cooled, diſcovered an evident whiteneſs imparted to both of them. One of the pieces afterwards looked like dull ſilver. Theſe two teſts have generally been thought ſufficient to diſtinguiſh the preſence of arſenic in any bodies, but I made uſe of a third, which has lately been communicated to the world by Mr. Bergman, and which is ſuppoſed to be in all *caſes* infallible.

I infuſed a ſmall quantity of the powder in a ſolution of a vegetable alkali in water for a few hours, and then poured it upon a ſolution of blue vitriol in water. The colour of the vitriol was immediately changed to a beautiful green, and afterwards precipitated.

I ſhall cloſe this paper with a few remarks upon this powder, and upon the cure of cancers and foul ulcers of all kinds.

The

1. The uſe of cauſtics in cancers and foul ulcers is very ancient, and univerſal. But I believe *arſenic* to be the moſt efficacious of any that has ever been uſed. It is the baſis of Plunkett's and probably of Guy's well known cancer powders. The great art of applying it ſucceſsfully, is to dilute and mix it in ſuch a manner as to mitigate the violence of its action. Dr. Martin's compoſition was happily calculated for this purpoſe. It gave leſs pain than the common or lunar cauſtic. It excited a moderate inflammation, which ſeparated the morbid from the ſound parts, and promoted a plentiful afflux of humours to the ſore during its application. It ſeldom produced an eſcar; hence it inſinuated itſelf into the deepeſt receſſes of the cancers, and frequently ſeparated theſe fibres in an unbroken ſtate which are generally called the roots of the cancer. Upon this account, I think, in an ulcerated cancer it is to be preferred to the knife. It has no action upon the ſound ſkin. This Dr. Hall proved by confining a ſmall quantity of it upon his arm for many hours. In thoſe caſes where Dr. Martin uſed it to extract cancerous or ſchirrous tumors that were not ulcerated, I have reaſon to believe that he always broke the ſkin with Spaniſh flies.

2. The arſenic uſed by the doctor was the pure white arſenic. I ſhould ſuppoſe from the examination I made of the powder with the eye, that the proportion of arſenic to the vegetable powder, could not be more than $\frac{1}{10}$ part of the whole compound. I have reaſon to think that the doctor employed different vegetable ſubſtances at different times. The vegetable matter with which the arſenic was combined in the powder which I uſed in my experiments, was probably nothing more than the powder of the root and berries of the ſolanum lethale, or deadly nightſhade. As the principal, and perhaps the only deſign of the vegetable addition was to blunt the activity of the arſenic, I ſhould ſuppoſe that the ſame proportion

tion of common wheat flour as the doctor uſed of his cauſtic vegetables, would anſwer nearly the ſame purpoſe. In thoſe caſes where the doctor applied a feather dipped in a liquid to the ſore of his patient, I have no doubt but his phial contained nothing but a weak ſolution of arſenic in water. This is no new method of applying arſenic to foul ulcers. Dr. Way of Wilmington, has ſpoken in the higheſt terms to me of a waſh for foulneſſes on the ſkin, as well as old ulcers, prepared by boiling an ounce of white arſenic in two quarts of water to three pints, and applying it once or twice a day.

3. I mentioned formerly that Dr. Martin was often unſucceſsful in the application of his powder. This was occaſioned by his uſing it indiſcriminately in *all* caſes. In ſchirrous and cancerous tumours, the knife ſhould always be preferred to the cauſtic. In cancerous ulcers attended with a ſcrophulous or a bad habit of body, ſuch particularly as have their ſeat in the neck, in the breaſts of females, and in the axillary glands, it can only protract the patient's miſery. Moſt of the cancerous ſores cured by Dr. Martin were ſeated on the noſe, or cheeks, or upon the ſurface or extremities of the body. It remains yet to diſcover a cure for cancers that taint the fluids, or infect the whole lymphatic ſyſtem. This cure I apprehend muſt be ſought for in diet, or in the long uſe of ſome internal medicine.

To pronounce a diſeaſe incurable, is often to render it ſo. The intermitting fever, if left to itſelf, would probably prove frequently, and perhaps more ſpeedily fatal than cancers. And as cancerous tumours and ſores are often neglected, or treated improperly by injudicious people, from an apprehenſion that they are incurable, (to which the frequent advice of phyſicians "to let them alone," has no doubt contributed) perhaps the introduction of arſenic into regular practice as a remedy for cancers, may invite to a more early application to phyſicians, and thereby prevent

vent the deplorable caſes that have been mentioned, which are often rendered ſo by delay or unſkilful management.

4. It is not in cancerous ſores only that Dr. Martin's powder has been found to do ſervice. In ſores of all kinds, and from a variety of cauſes, where they have been attended with fungous fleſh or callous edges, I have uſed the doctor's powder with advantage.

I flatter myſelf that I ſhall be excuſed in giving this detail of a *quack* medicine, when the ſociety reflect that it was from the inventions and temerity of quacks, that phyſicians have derived ſome of their moſt active and uſeful medicines.

N° XXVII.

Illuſtriſſimæ ac celeberrimæ Societati Scientiarum quæ eſt Philadelphiæ.

S. P. D.

CHRISTIANUS MAYER Ser^mi Electoris Palatini Aſtronomus.

SCRIBENDI occaſionem a Cl. D. Ferdinando Farmer oblatam eo minus negligendam putavi quod hac ratione aliquantum reſpondeam honori, quo me illuſtriſſima ſocietas affecit, cum me in album ſuorum ſociorum ad ſcripſit. Ex libro Philadelphiæ impreſſo & ad me tribus circiter abhinc annis tranſmiſſo intellexi non ſine magno animi mei ſenſu, etiam Philadelphiæ excoli aſtronomiam. Libro illo ſcriptiſque meis aſtronomicis infelici incendio abhinc biennio conſumtis, de novis meis quibuſdam in cœlo inventis ad ſocietatem illuſtriſſimam aliquid ſcribendum eſſe, duxi. Speculam novam ad omnes uſus accommodatam

tam Manhemii inhabito; nec deſunt pretioſiſſima Londinenſia inſtrumenta, in quibus præcipue eminet quadrans muralis æneus 8 pedum in rhadio a cel. artifici Bird anno 1775 confectus & plane inſigni tubo achromatico inſtructus, ſolidiſſimeque muro affixus ad plagam cœli meridionalem, quo inſtrumento, quoties cœlum favet, utor quotidie. Adverti autem ſtatim abhinc biennio in ſtellis fixis plane multis a primo gradu magnitudnis ad ſextum uſque, diſtingui alias ſtellulas parvulas comites, quarum aliæ ob lucem tranquillam & obtuſam planetarum ſpeciem referunt, aliæ teleſcopicam parvitatem non excedunt. Quod maxime mirabar, illud eſt, quod has ſtellulas comites, pauciſſimis duntaxat exceptis, nullo noto catalogo contineri viderem, cum tamen earum uſum ad determinandum motum proprium fixarum eſſe plane inſignem evidenter colligerem. Cum enim ibi, ubi paucorum plerumqe ſecundorum reperitur differentia aſcenſionis rectæ & declinationis inter fixam lucidiorem, ejuſque comitem, lapſus temporis haud aliam variationem ſtellæ fixæ, quam ejus comiti inducere poſſit, unde demum cunque iſta mutatio oriatur, ſive a præceſſione æquinoctiorum, ſive a variatione obliquitatis eclipticæ, ſive a deviatione inſtrumenti, ſeu ab aberratione luminis aut nutationis, ſive ab alia quacunque cauſa, quæ pendeat a mutabili ſtatu atmoſpheræ aut locorum latitudine, contingit ſane, ut omnis mutatio viſa inter fixam, ejuſque comitem, motus proprii argumentum præbeat certiſſimum, ſive is fixam ſive ejus comitem afficiat. Noveram Halleum cel. Angliæ aſtronomum primum fuiſſe, qui anno 1719 ex inſtituta comparatione obſervationum Flamſteadii cum illis Ptolomæi in paucis quibuſdam fixis, Syrio, Arcturo & Aldebaran deprehendit has ſtellas moveri motu ſingulari proprio. Sed ſimul noveram in Hiſtoria Cœleſti Brittannica Flamſteadii jam anno 1690 uſurpatam fuiſſe a Flamſteadio vocem comitis fixarum, cum vir ſummus nec dum de motu proprio fixarum cogitaſſet. Aſtronomi reliqui Halleo poſteriores quotquot in motum proprium fixarum inquiſiverunt,

inquiſiverunt, Hallei methodum ſecuti ſunt, comparando obſervationes ſuas cum obſervationibus antiquorum: methodus hæc prolixos requirit calculorum labores, multiſque dubitationibus manet obnoxia ob incertitudinem, lubricamque conditionem inſtrumentorum, & obſervationum antiquarum; non item methodus mea nova, qua ex variatione inter comitem & fixam illuſtriorem obſervata, ſtatim conſequitur dari motum proprium, vel utriuſque vel allerutrius ſideris. Itaque ducentos fere diverſarum fixarum comites a biennio obſervavi, eundem fere paralellum ſtatim ante vel poſt fixam decurrentes, & obſervationes hujuſmodi plures cum cel. Angliæ aſtronomo Nevil Maſkelyne communicavi, qui eas ſibi gratiſſimas accidiſſe reſpondet. Ex multis, obſervationes paucas ad illuſtriſſimam ſocietatem ſpeciminis loco tranſmitto, quarum reſpondentes in Hiſtoria Cœleſti Britannica Flamſteadii invenio, unde ſimul patet, quam obſervationes hujuſmodi præclare ſerviant motui proprio detegendo. Prima et ſecunda columna ſiniſtima tabulæ ſequentis ex titulo facillime intelligitur. Tertia columna differentiam A. R. in tempore medio indicat inter ſtellam ejuſque comitem: comes præcedens ſixam, primo loco in tabula ſcribitur, comes ſequens poſt fixam ponitur. Quarta columna differentiam declinationis inter fixam ejuſque comitem notat, qualem ego Manhemii obſervavi. Litera A ſignificat comitem eſſe auſtraliorem, litera B magis borealem. Sequentes columnæ, obſervationes ejuſdem ſtellæ factas a Flamſteadio, continent.

Mayer, Manheim.				Flamstaed, Greenwich.		
	Nomina Fixarum.	Diff. A. R. in tempore.	Differ. Declina.	Diff. A. R.	Differ. Declina.	
1777 Die 28 Feb.	ρ Leonis Comes 7æ. five 8væ.	2′. 15″.	38′. 58″. 6. A	2′. 18″.	38′. 50″. A	1690 Die 25 Mar.
1777 28 Feb. item 23 Feb.	Com. γ Leonis 6tæ. γ Leonis 4tæ	5″.	21′. 57″. 2. A	2″.	22′. 5″.	1691 6 Aprilis.
1777 4 Aprilis.	Comes τ Leonis	1′. 5″.	8′. 0″. 15. B	56.	8′. 30″. B	1690 25 Martii.
1777 11 Martii.	Propus Comes 5tæ	5′. 36″.	7′. 30.″	5′. 38″.	7′. 0″.	1690 7 Feb.
1777 8 Martii.	Procyon Comes II. 8væ	1′. 40″. 5.	3′. 6″. 2. A	1′. 34″.	4′. 40″. A	1691 4 Octo.
1777 13 Nov.	Com. 7mæ ε Piſcium	51″.	2′. 57″. 1. B	47″.	2′. 30″. B	1700 Nov. 13.
1777 Aprilis 6.	Comes Π Cancri	2′. 49″.	1′. 40″. B	2′. 44″.	1′. 12″. B	1690 Feb. 18.
1777 Aprilis 8.	Com. 7æ β Leonis	30″.	18′. 27″. 1. A	33″½		1692 Aprilis 24.
1776 Mai. 18.	Arcturus Comes.	0. 0. 6″.	0. 23. 37. 3	Com. 0. 5″. Arcturus.	0. 26. 30.	1690 Febru. 14.
1777 Mart. 11.	Procyon Comes 7æ.	39″.	3′. 8″. 1. A	35″.		1692 22 Janu.
1776 Mai. 18.	Arcturus Comes.	0. 0. 6.	0. 23. 37. 4	Maskelyne. A. R. 0. 0. 4.	0. 23′. 58″. 8.	1765 20 Mai.
1777 Aprilis 1.	O Cancri ſeg. Π Cancri 6.	14′. 56″.	32. 29. 6. B	15. 1.	32. 25. B	1691 10 Martii.

Apparet ex omnibus Arcturum omnium celerrime ferri motu proprio in occaſum, ſequidem idem comes, qui tempore Flamſteadii 1690, die 14 Februarii Arcturum præcedebat 5″ in tempore nunc 6″ poſt Arcturum meridianum ingreditur: ex imminuta quoque differentia declinationis inter Arcturum ejuſque comitem, patet, Arcturum motu proprio quotannis fere 2″ in circulo moveri verſus auſtrum. Id ex eo perſpicuum eſt, quod declinatio a me obſervata comitis reducta ad parallelum grenovicenſem eandem producat altitudinem poli grenovicenſis, qualis ex obſervatione Flamſteadii eruitur, non item declinatio hodie obſervata Arcturi etiam aberratione & nutatione correcta. Similis indagatio fieri poteſt in reliquis fixis, earumque comitibus, atque ex inſtituta comparatione cum aliis fixis deprehendi

deprehendi poteſt, num fixæ an comiti vel utrique motus proprius tribuendis ſit.

Obſervationes omnes in plano meridiani quadrante murali factæ ſunt Manhemii in nova ſpecula a me ædificata S. Electoris Palatini: ejus longitudo ad ortum Grenovicii eſt fere 34′ 6″ in tempore, latitudo fere 49° 27′ 50″ Gaudebo maxime ſi has meas obſervationes illuſtriſſimæ ſocietati haud ingratas accidiſſe intellexero, cujus benevolentiæ me demiſiſſime commendo.

Illuſtriſſimæ ac celeberrimæ Societati,
Cuetor et ſervus perpetuus,
CHRISTIANUS MAYER, Sereniſſimi
Electoris Palatini et Ducis Bavariæ Aſtronomus.

Manhemii in Germania,
die 24 Aprilis 1778.

(TRANSLATION.)

To the illuſtrious and celebrated Society of Sciences at Philadelphia,

CHRISTIAN MAYER, Aſtronomer to his Serene Highneſs the Elector Palatine,

Wiſheth Proſperity.

I THOUGHT it my duty to embrace the opportunity which my good friend the Rev. Mr. Ferdinand Farmer has procured me of writing to your illuſtrious ſociety, that I may make ſome return to the honour which you have done me by electing and enrolling me among your members. It gave me a very ſenſible pleaſure to find, by the printed volume of tranſactions, which you ſent me about three years ago, that the ſcience of aſtronomy was cultivated even at Philadelphia. That volume of yours, together

gether with my own aſtronomical papers, having been unhappily deſtroyed by fire about two years ago, I have reſolved to give your illuſtrious ſociety ſome ſhort account of certain new celeſtial diſcoveries which I had made.

My reſidence is now at Manheim, in a new obſervatory, fitted for every aſtronomical purpoſe; and well furniſhed with the moſt precious and accurate inſtruments made at London; amongſt which the chief is a braſs mural quadrant of eight feet radius, the workmanſhip of that celebrated artiſt Mr. Bird, finiſhed in the year 1775, fitted with an achromatic teleſcope, and fixed to a ſolid wall towards the meridian. With this inſtrument I make daily obſervations of the heavens, when the weather will permit, and two years ago I diſtinctly diſcovered, among many of the *fixed ſtars* (from the *firſt* to the *ſixth* magnitude) other *concomitant* or *attendant little ſtars*; ſome of which, from their mild, faint (or unſparkling) light, have the appearance of planets, while others of them have the appearance of teleſcopic ſtars, in reſpect to their ſmallneſs.

But what ſurpriſed me moſt was, that none of theſe *attendant* little ſtars, a few perhaps excepted, have ever been noted in any catalogue which I have ſeen; although I could clearly collect the ſingular uſe which may be made of them for aſcertaining and determining the proper motion of the fixed ſtars, as it is called. When the difference of right aſcenſion and declination between two ſtars is at moſt but a few ſeconds, any variation ariſing from the preceſſion of the equinoxes, the variation of the obliquity of the ecliptic, the deviation of the inſtrument, the aberration of light or the nutation, or from any other cauſe depending on the mutable ſtate of the air or latitude of places, muſt affect them both equally. Therefore when after any length of time a greater variation of right aſcenſion or declination is found in one of ſuch ſtars than in the other, it affords a certain argument of the proper motion of one or the other, whether that change affects the fixed ſtar or its attendant.

I know

I know that the celebrated Engliſh aſtronomer Halley, was the firſt who, about the year 1719, by a careful compariſon of the obſervations of Flamſtead with thoſe of Ptolemy, reſpecting a few fixed ſtars, viz. Sirius, Arcturus and Aldebaran, diſcovered that theſe ſtars had a proper motion of their own. But I likewiſe know, that in Flamſtead's Britiſh celeſtial hiſtory the word *concomitant or attendant of fixed ſtars* is made uſe of, when that great man had not even thought of a proper motion of the fixed ſtars.

The other aſtronomers, poſterior to Halley, as far as they inveſtigated the proper motion of the *fixed ſtars*, followed the Italian method of comparing their own obſervations with thoſe of the ancients. This method requires the labour of prolix calculation, and remains liable to doubts and uncertainty, on account of the inaccuracy of ancient inſtruments and obſervations. My new method is not liable to ſuch objections, becauſe from the obſerved variation of the attendant ſtar and the brighter fixed ſtar, it immediately follows that there is a proper motion given, either of the one or the other.

I have, therefore, in the ſpace of two years, obſerved almoſt two hundred attendants of different fixed ſtars, running almoſt the ſame parallel, immediately before or after the fixed ſtar; and have communicated many of ſuch obſervations to the celebrated Engliſh aſtronomer Nevil Maſkelyne, who has expreſſed his high ſatisfaction therewith.

Out of many obſervations, I ſend your illuſtrious ſociety a few by way of ſpecimen, being ſuch whereof I find correſpondent obſervations in the Flamſteadian celeſtial hiſtory; whence it appears at once how excellently obſervations of this kind ſerve for diſcovering the proper motion of ſuch ſtars.

The firſt and ſecond column of the following table next to the left hand is eaſily underſtood from its title. The third column ſhews the difference of A. R. in mean time

between the *attendant* and *ſtar*. The attendant which precedes the fixed ſtar occupies the firſt place in the table. The ſubſequent attendant is placed after the fixed ſtar. The fourth column ſhews the difference of declination between the fixed ſtar and attendant. The letter A, ſignifies that the attendant is more ſouth, B, that it is more north than the ſtar. The ſubſequent columns contain Flamſtead's obſervations of the ſame fixed ſtars.

The TABLE.

By Mayer, at Manheim.				By Flamstead, at Greenwich.		
Time of Obſervation.	Names of Star.	Diff. A. R. in time.	Differ. of Declina.	Diff. A. R. in time.	Differ. of Declina.	Time of Obſervation.
1777 Februar. 28.	ρ Leonis attendant 7m.	2′. 15″.	38′. 58″. 6. A	2′. 18″.	38′. 50″. A	1690 March 25.
1777 Feb. 25, 28.	Attend. 6m. γ Leonis 4m.	5″.	21′. 57″. 2. A	2″.	22′. 5″.	1691 6 April.
1777 April 4.	Attendant, τ Leonis	1′. 5″.	8′. 0″. 15. B	56.	8′. 30″. B	1690 25 March.
1777 March 11.	Propus attendant 5m.	5′. 36″.	7′. 30.″	5′. 38″.	7′. 0″.	1690 7 Feb.
1777 March 8.	Procyon attendant 8m.	1′. 40″. 5.	3′. 6″. 2. A	1′. 34″.	4′. 40″. A	1691 4 Octo.
1777 13 Nov.	Attendant 7m. ε Piſcium	51″.	2′. 57″. 1. B	47″.	2′. 30″. B	1700 Nov. 13.
1777 April 6.	Attendant, Π Cancri	2′. 49″.	1′. 40″. B	2′. 44″.	1′. 12″. B	1690 Feb. 18.
1777 April 8.	Attendant 7m. β Leonis	30″.	18′. 27″. 1. A	33″½		1692 April 24.
1776 May 18.	Arcturus Attendant,	0. 0. 6″.	0. 23. 37. 3	Com. 0. 5″. Arcturus.	0. 26. 30.	1690 Febru. 14.
1777 March 11.	Procyon Attendant 7m.	39″.	3′. 8″. 1. A	35″.		1692 22 Janu.
1776 May 18.	Arcturus Attendant,	0. 0. 6.	0. 23. 37. 4	Maskelyne. A. R. 0. 0. 4.	0. 23′. 58″. 8.	1765 20 May.
1777 April 1.	O Cancri Π Cancri 6m.	14′. 56″.	32. 29. 6. B	15. 1.	32. 25. B	1691 10 March.

It appears from every obſervation, that of all the ſtars, Arcturus, by his proper motion, is carried with the greateſt celerity weſtward; ſince the ſame attendant, which in Flamſtead's

Flamſtead's time, 1690, February 14, preceded Arcturus 5″ in time, now comes to the meridian 6″ after him. Likewiſe from the very ſmall difference of declination between Arcturus and his attendant, it appears that Arcturus by his proper motion moves almoſt 2″ ſoutherly every year in a circle.

N° XXVIII.

Obſervations on the Cauſe and Cure of the Tetanus, by BENJAMIN RUSH, M. D. *Profeſſor of Chemiſtry in the Univerſity of Pennſylvania.*

Read Mar. 17, 1786. DURING my attendance upon the military hoſpitals of the United States, in the courſe of the late war, I met with ſeveral caſes of the Tetanus. I had frequently met with this diſorder in private practice, and am ſorry to ſay that I never ſucceeded with the ordinary remedy of opium in any one caſe that came under my care. I found it equally ineffectual in the army. Baffled in my expectations from a remedy that had been ſo much celebrated, I began to inveſtigate more particularly the nature of the diſorder. I found it to be a diſorder of warm climates, and warm ſeaſons. This led me to aſcribe it to relaxation. I reſolved to attempt the cure of it by a ſet of medicines in ſome meaſure the oppoſites of moſt of the medicines that had been employed in that diſorder. Soon after I adopted this reſolution, I was called to viſit Col. John Stone, who was wounded through the foot at the battle of Germantown on the 4th of October 1777. He was in the third day of a Tetanus. His ſpaſms were violent and his pains ſo exquiſite that his cries were heard near a hundred yards from his quarters. His head was thrown a little backwards, and his jaw had become ſtiff and contracted.

He

He was under the care of a ſkilful regimental ſurgeon who was pouring down opium in large quantities without effect.

Duty and friendſhip both led me to do my utmoſt to ſave the life of this valuable officer. I immediately diſmiſſed the opium, and gave him large quantities of wine and bark, to the amount of two or three ounces of the latter, and from a bottle to three pints of the former in the day. In a few hours I was delighted with their effects. His ſpaſms and pains were leſs frequent and violent, and he ſlept for ſeveral hours, which he had not done for ſeveral days and nights before.

With the ſame indication in view, I applied a bliſter between his ſhoulders, and rubbed in two or three ounces of mercurial ointment upon the outſide of his throat. He continued to mend gradually under the operation of theſe medicines, ſo that in ten days he was out of danger, although the ſpaſm continued in his wounded foot for ſeveral weeks afterwards. In the ſummer of the year 1782 I was called to viſit a ſervant girl of Mr. Alexander Todd, merchant of this city, who had brought on a Tetanus by ſleeping in the evening on a damp brick pavement, after a day in which the mercury in Farenheit's thermometer had ſtood at near 90°. The caſe was nearly as violent and alarming as the one I have deſcribed. I treated her in the ſame manner, and with the ſame ſucceſs. To the above named medicines, I added only the oil of amber which ſhe took in large doſes, after I ſuſpected the tonic powers of the bark and wine began to looſe their effects. The good effects of the oil were very obvious. She recovered gradually and has continued ever ſince in good health. In the ſummer of the ſame year I was called to Alexander Leſlie, a joiner, who had run a nail in his foot. I found him the day afterwards in extreme pain, with ſmall convulſions and now and then a twinge in his jaw. The wound in his foot was without

ſwelling

ſwelling or inflammation. I dilated the wound and filled it with lint moiſtened with ſpirit of turpentine. This in a little while produced a good deal of pain and a great inflammation in his foot. While I was preparing to treat him in the manner I had treated the two former caſes, the pains and ſpaſms in his body ſuddenly left him, and in twenty-four hours after I ſaw him, he complained of nothing but of the pain and ſwelling in his foot, which continued for ſeveral weeks and did not leave him till it ended in a ſuppuration. From the hiſtory of theſe three caſes, I beg leave to make the following remarks.

1. That the prediſpoſition to the Tetanus depends upon relaxation. This relaxation is generally produced by heat; but exceſſive labour, watchings, marches, or fatigue from any cauſe, all produce it likewiſe, and hence we find it more frequent from wounds received in battles, than from ſimilar wounds received in any other way. Theſe wounds more certainly produce the Tetanus, if they have been preceded for ſome time with warm weather. Dr. Shoepft, the phyſician general of the Anſpach troops that ſerved at the ſiege of York in the year 1781, informed me of a ſingular fact upon this ſubject. Upon converſing with the French ſurgeons after the capitulation, he was informed by them that the troops who arrived juſt before the ſiege from the Weſt-Indies with Count de Graſſe, were the only troops belonging to their nation that ſuffered from the Tetanus. There was not a ſingle inſtance of that diſorder among the troops who had ſpent a winter in Rhode-Iſland.

2. As the Tetanus ſeems to be occaſioned by relaxation, the medicines indicated to cure it are ſuch only as are calculated to remove this relaxation and to reſtore a tone to the ſyſtem. The bark and wine appear to act in this way. The operation of the bliſters is of a more complicated nature. That they are ſedative and antiſpaſmodic in fevers is univerſally acknowledged, but in the peculiar ſtate of irritability

irritability which occurs in the Tetanus, perhaps their effects are more ſimply ſtimulating. But I will go one ſtep further. In order to cure this diſorder, it is neceſſary not only to produce an ordinary tone in the ſyſtem, but ſomething like the inflammatory diatheſis. The abſence of this diatheſis is taken notice of by all authors, particularly by Dr. Cullen*.

Mercury appears to act only by promoting this diatheſis. Hence it never does any ſervice unleſs it be given time enough to produce a ſalivation. The irritation and inflammation produced in the mouth and throat, ſeldom fail to produce the inflammatory diatheſis, as blood drawn in a ſalivation has repeatedly ſhewn.

I apprehend that the oil of amber acts as a ſtimulant chiefly in this diſorder. I have heard of a Tetanus being cured in the iſland of Grenada by large doſes of muſtard. Dr. Wright, lately of the iſland of Jamaica, relates in the 6th volume of the London Medical Eſſays, ſeveral remarkable caſes of the Tetanus being cured by the cold bath. Both theſe remedies certainly act as ſtimulants and tonics. By reaſoning *à priori*, I conceive that electricity would be found to be an equally powerful remedy in this diſorder.

As a general inflammatory diatheſis diſpoſes to topical inflammation, ſo topical inflammation diſpoſes to general inflammatory diatheſis. Wounds upon this account are leſs apt to inflame in ſummer than in winter. In the Tetanus I have uniformly obſerved an abſence of all inflammation in the wounds or injuries that produced it. A ſplinter under the nail produces no convulſions, if pain, inflammation and ſuppuration follow the accident. It is by exciting pain and inflammation I apprehend that the ſpirit of turpentine acts in all wounds and punctures of nervous and tendinous parts. I have never known a ſingle inſtance of a Tetanus from a wound, where this remedy had been applied in time. It was to excite an inflammation in the foot of Mr. Leſlie, that I dilated the wound and

* Firſt Lines, Vol. III.

and filled it with the ſpirit of turpentine. I was not ſurpriſed at its good effects in this caſe, for I was prepared to expect them.

I find a remarkable caſe related in Dr. W. Monroe's Theſis, publiſhed in Edinburgh in the year 1783, of a black girl who had a Tetanus from running a nail in her foot, being perfectly cured by deep and extenſive inciſions being made in the wounded part by Dr. John Bell, of the iſland of Grenada.

It is by producing inflammation in a particular part, and tone in the whole ſyſtem, I apprehend that the amputation of a wounded limb ſometimes cures a Tetanus; and it is becauſe the degrees of both are too inconſiderable to oppoſe the violence of the ſpaſms in the advanced ſtages of the Tetanus, that amputation often fails of ſucceſs.

I have been informed by a phyſician who reſided ſome time at St. Croix, that the negroes on that iſland always apply a plaiſter made of equal parts of ſalt and tallow to their freſh wounds, in order to prevent a locked jaw. The ſalt always produces ſome degree of inflammation.

If the facts that have been ſtated are true, and the inferences that have been drawn from them are juſt, how ſhall we account for the action of opium in curing this diſorder? I do not deny its good effects in many caſes, but I believe it has failed in four caſes out of five in the hands of moſt practitioners. It is remarkable that it ſucceeds only where it is given in very large doſes. In theſe caſes I would ſuppoſe that its ſedative powers are loſt in its ſtimulating. It is upon a footing, therefore, in one reſpect, with the ſtimulating medicines that have been mentioned; but from its being combined with a ſedative quality, it is probably inferior to moſt of them. I am the more inclined to adopt this opinion, from an account I once received from Dr. Robert, of the iſland of Dominique, who informed me that after having cured a negro man of a Tetanus with large doſes of opium, he was afterwards ſeized

with a diſorder in his ſtomach, of which he died in a few days. Upon opening him, he found his ſtomach inflamed and mortified. I do not forbid the uſe of opium altogether in this diſorder. I think ſmall doſes of it may be given to eaſe pain, as in other ſpaſmodic diſorders; but as its qualities are complicated, and its efficacy doubtful, I think it ought to yield to more ſimple and more powerful remedies.

To the caſes that have been mentioned, I could add many others, in which I have reaſon to believe that the excitement of a topical inflammation by artificial means, has effectually prevented a Tetanus.

To this account of the Tetanus, I beg leave to ſubjoin a few words upon a diſorder commonly called the jaw-fall in infants, or the Triſmus Naſcentium of Dr. Cullen, which is nothing but a ſpecies of Tetanus.

I have met with three caſes of it in this city, all of which proved fatal. The ſtage of the diſorder in which I was conſulted, and the age and weakneſs of the infants, forbad me to attempt any thing for their relief. I have introduced the ſubject of this diſorder in children, only for the ſake of mentioning a fact communicated to me by the late Dr. Cadwalader Evans of this city. This gentleman practiſed phyſic for ſeveral years in Jamaica, where he had frequent opportunities of ſeeing the Tetanus in the black children. He found it in every caſe to be incurable. He ſuppoſed it to be occaſioned by the retention of the meconium in the bowels. This led him invariably to purge every child that was born upon the eſtates committed to his care. After he adopted this practice, he never met with a ſingle inſtance of the Tetanus among children.

Perhaps it may tend to enlarge our ideas of the Tetanus, and to promote a ſpirit of enquiry and experiment, to add, that this diſorder is not confined to the human ſpecies. I have known ſeveral inſtances of it in horſes from nails running

running in their feet, and other accidents. It is attended with a rigidity of the muſcles of the neck, a ſtiffneſs in the limbs, and ſuch a contraction of the jaw as to prevent their eating. It is generally fatal. In two caſes I had the pleaſure of ſeeing the diſeaſe perfectly cured by applying a potential cauſtic to the neck under the mane, by large doſes of oil of amber, and by plunging one of them into the river, and throwing buckets of cold water upon the other.

How far the reaſonings contained in this paper may apply to the hydrophobia, I cannot determine, having had no opportunity of ſeeing the diſeaſe ſince I adopted theſe principles; but from the ſpaſmodic nature of the diſorder, from the ſeaſon of the year in which it generally occurs, and above all, from the caſe related by Dr. Fothergill, of a young woman having eſcaped the effects of the bite of a mad cat by means of the wound being kept open, (which from its ſeverity was probably connected with ſome degrees of inflammation) is it not probable that the ſame remedies, which have been uſed with ſucceſs in the Tetanus, may be uſed with advantage in the hydrophobia?—In a diſeaſe ſo deplorable, and hitherto ſo unſucceſsfully treated, even a conjecture may lead to uſeful experiments and enquiries.

N° XXIX.

To His Excellency BENJAMIN FRANKLIN, *Eſq. L. L. D. Preſident of the State of Pennſylvania, and of the American Philoſophical Society, &c.*

SIR, Philadelphia, January 12, 1786.

Read March 17, 1786. THE ſubject of ſmoky chimneys, of which I had the honor of converſing with you at your own houſe laſt evening, is of ſo much importance to

every individual, as well as to every private family, that too much light cannot be thrown upon it.

A ſmoky houſe and a ſcolding wife,
Are (ſaid to be) two of the greateſt ills in life.

And however difficult it may be to remedy one of thoſe ills, yet any advances we may be able to make towards removing the inconveniencies ariſing from the other, cannot fail to be favourably received by the public. As they are ſhortly to be favoured with your ſentiments on that ſubject, poſſibly the following obſervations, which were in fact occaſioned by neceſſity, and are the reſult of my own experience, may not be altogether undeſerving of notice.

When I left London and went to live in Devonſhire in the latter end of the year 1777, it happened to be my lot to dwell in an old manſion which had been recently moderniſed, and had undergone a thorough repair. But as in moſt of the old houſes in England, the chimneys, which were perhaps originally built for the purpoſe of burning wood, though they had been contracted in front, ſince coal fires came into general uſe, to the modern ſize, yet they were ſtill, above, out of ſight, extravagantly large. This method of building chimneys may perhaps have anſwered well enough while it was the cuſtom to ſit with the doors and windows open; but when the cuſtoms and manners of the people began to be more poliſhed and refined, when building and architecture were improved, and they began to conceive the idea of making their chambers cloſe, warm, and comfortable, theſe chimneys were found to ſmoke abominably, for want of a ſufficient ſupply of air. This was exactly the caſe with the houſe in which I firſt lived, near Exeter, and I was under the neceſſity of trying every expedient I could think of to make it habitable.

The firſt thing I tried, was that method of contracting the chimneys by means of earthen pots, much in uſe in England,

England, which are made on purpoſe, and which are put upon the tops of them; but this method by no means anſwered. I then thought of contracting them below, but as the method of contracting them in front to the ſize of a ſmall coal-fire grate has an unſightly appearance, as it makes a diſagreeable blowing like a furnace, and as it is the occaſion of conſuming a great deal of unneceſſary fuel, the heat of which is immediately hurried up the chimney, I rejected this method, and determined to contract them above, a little out of ſight. For this purpoſe I threw an arch acroſs, and alſo drew them in at the ſides. This had ſome effect, but as this contraction was made rather ſuddenly, and the ſmoke, by ſtriking againſt the corners that were thereby occaſioned, was apt to recoil, by which means ſome part of it was thrown out into the room; I determined to make the contraction more gradually, and therefore run it up at the back, where the depth of the chimney would admit of it, and alſo ſhelving or ſloping in a conical kind of direction at the ſides, as high as a man, ſtanding upright, could conveniently reach, and by this means brought the cavity within the ſpace of about twelve by fourteen or ſixteen inches, which I found ſufficiently large to admit a boy to go up and down to ſweep the chimnies. This method I found to ſucceed perfectly well, as to curing the chimneys of ſmoking, and it had this good effect of making the rooms conſiderably warmer; and as this experiment ſucceeded ſo well, ſince the only uſe of a chimney is to convey away the ſmoke, I determined to carry it ſtill farther, in order to aſcertain with preciſion, how much ſpace is abſolutely neceſſary for that purpoſe, becauſe all the reſt that is ſhut up, muſt be ſo much gained in warmth. Accordingly I laid a piece of ſlate acroſs the remaining aperture, removable at pleaſure, ſo as to contract the ſpace above two thirds, leaving about three inches by twelve remaining open; but this ſpace, except when the fire burnt remarkably clear, was

ſcarcely

ſcarcely ſufficient to carry away the ſmoke. I therefore enlarged it to half the ſpace, that is, to about ſix by ſeven or eight inches, which I found fully ſufficient to carry away the ſmoke from the largeſt fires.

When I removed into the Bedford Circus in Exeter, though the houſe was modern, and almoſt perfectly new, yet the chimneys were large; in conſequence of which almoſt every room of it ſmoked. My predeceſſor, who was the firſt inhabitant, had been at great expence in patent ſtoves, &c. but without effect; but by adopting the method I have juſt now deſcribed, I not only cured every chimney of ſmoking, but my houſe was remarked for being one of the warmeſt and moſt comfortable to live in of any in that large and opulent city.

The houſe I now live in, in Philadelphia, I am told, has always had the character of being both cold and ſmoky; and I was convinced, as ſoon as I ſaw the rooms and examined the chimneys, that it deſerved that character; for though the rooms were cloſe, the chimneys were large: And we ſhall ever find, that if our chimneys are large, our rooms will be cold even though they ſhould be tolerably cloſe and tight; becauſe the conſtant ruſhing in of the cold air at the cracks and crevices, and alſo at every opening of the door will be ſufficient to chill the air, as faſt as it is heated, or to force the heated air up the chimney; but by contracting the chimneys I have cured it of both theſe defects. There was one remarkable circumſtance attending the contraction of the chimney in the front parlour, which deſerves to be attended to; which was, that before I applied the caſt iron plate, which I made uſe of inſtead of ſlate, to diminiſh the ſpace requiſite for a chimney ſweeper's boy to go up and down, the ſuction or draught of air was ſo great, that it was with difficulty I could ſhut the door of the room, inſomuch that I at firſt thought it was owing to a tightneſs of the hinges, which I imagined muſt be remedied, but upon applying the

the iron plate, by which the ſpace was diminiſhed one half, the door ſhut to with the greateſt eaſe. This extraordinary preſſure of the air upon the door of the room, or ſuction of the chimney, I take to be owing in ſome meaſure to the unuſual height of the houſe.

Upon the whole, therefore, this fact ſeems clearly aſcertained, viz. That the flue or ſize of the chimney, ought always to be proportioned to the tightneſs and cloſeneſs of the room, ſome air is undoubtedly neceſſary to be admitted into the room in order to carry up the ſmoke, otherwiſe as you juſtly obſerved we might as well expect ſmoke to ariſe out of an exhauſted receiver; but if the flue is very large, and the room is tight, either the ſmoke will not aſcend, in conſequence of which will be, that the air of your room will be ſo frequently and ſo conſtantly changed that as faſt as it is heated, it will be hurried away, with the ſmoke, up the chimney, and of courſe your room will be conſtantly cold.

One great advantage attending this method of curing ſmoky chimneys is, that, in the firſt place, it makes no aukward or unſightly appearance, nothing being to be ſeen but what is uſual to chimneys in common; and in the ſecond place that it is attended with very little expence, a few bricks and mortar with a plate or covering to the aperture, and a little labour, being all that is requiſite. But in this new country where crops of houſes may be expected to riſe almoſt as quick as fields of corn, when the principles upon which chimneys ought to be thoroughly underſtood, it is to be hoped, that not only this expence, ſmall as it is, but that all the other inconveniencies we have been ſpeaking of, will be avoided, by conſtructing the flues of the chimneys ſufficiently ſmall.

From your humble ſervant,

THOMAS RUSTON.

N° XXX.

Obſervations on the annual Paſſage of Herrings, by Mr. JOHN GILPIN.

AS this very uſeful part of the finny race has never been found in the freſh rivers, or waters of Europe, it remains a query amongſt the naturaliſts, where they go to ſpawn and perpetuate their ſpecies. I apprehend this query may be anſwered to the ſatisfaction of the curious by an account of their annual progreſs, from which it will appear they are a fiſh of paſſage, and obſerve one regular annual rout in the ſea, ſhifting their climate with the ſun, and that it is the ſame ſcoole which is found at different times about Britain and in America. This opinion is founded on obſervations made on ſeeing them caught at Whitehaven and in this country, from which I have not obſerved that there is any viſible difference in the fiſh in the different places, except that thoſe at Whitehaven are fatter and rounder than thoſe in America; but this difference is not ſo great as that between the ſpring and fall mackarel, and which I conceive might be accounted for from the time of the year, and manner in which they appear on each coaſt. For they are found on the other ſide the Atlantic, or rather in the North ſea, in the favourable month of June about the iſlands of Shetland, from whence they proceed down to the Orkneys, and then dividing, they ſurround the iſlands of Great-Britain and Ireland, and unite again off the Land's End in the Britiſh channel in September, from whence this grand united ſcoole ſteers ſouth-weſt, and is not found any more on that ſide or in the Atlantic, until the ſame time the enſuing year, but appear next on the American coaſts.

coaſts. They arrive in Georgia and Carolina the latter end of January, and in Virginia in February; and coaſting from thence eaſtward to New-England, they divide and go into all the bays, rivers, creeks and even ſmall ſtreams of water in amazing quantities, and continue ſpawning in the freſh water until the latter end of April, when the old fiſh return into the ſea, where they change their latitudes by a northward direction and arrive at Newfoundland in May; after which we neither hear or ſee any thing more of them in America, until their return amongſt us the enſuing ſpring, and bring with them a providential bleſſing to the poor. Their coming ſooner or later up our rivers depends on the warmth or coolneſs of the ſeaſon: And it is further obſerved that if a few warm days invite them up, and cool weather ſucceeds, it totally checks their paſſage until more warm weather returns. From all which circumſtances it appears probable there is a certain degree of warmth particularly agreeable to them, which they endeavour to enjoy by changing their latitude according to the diſtances of the ſun. Thus they are found in the Britiſh channel in September, but leave it when the ſun is at too great a diſtance from them in the ſouthern hemiſphere, and puſh for a more agreeable climate; and when the weather in America becomes too warm in May, (after having depoſited their eggs in ſhallow water and ſecured their young fry from the fiſh of prey,) ſteer the courſe which leads to the cooler northern ſeas, and by that prudent change of place perpetually enjoy the temperature of climate beſt adapted to their nature; which from the table hereto annexed, ſhewing the places and times of their viſitation, and the calculation of the diſtance of the ſun at thoſe times from them, is that degree of warmth which is produced by the mean diſtance between 37 to 43 degrees; except whilſt they are ſpawning; during which they bear a greater degree of heat from the neceſſity of remaining in it a ſhort time to ſpawn; and alſo on the other extreme, when detained at too great a diſtance by the iſland of Great-Britain and its dependencies.

Here another query occurs, what becomes of the young fry, the produce of the ſpawn they left in the freſh waters of America? We know they do not follow the old ones the firſt ſeaſon, becauſe they are found in great ſcooles in all the American bays during the ſummer, and diſappear in the fall, from whence it may rationally be ſuppoſed that from their natural propenſity to keep at a certain diſtance from the ſun, the ſeaſon leads them to a different courſe from the old ones, by which they meet their parentage about the latitude 23° N. and 70° W. longitude, and there tack about and follow the older ones; which, being larger and ſtronger than the younger, come firſt into our harbours, but are fewer in number than the leſſer, probably from having ſuffered great loſs and pillage in their long rout from the fiſh of prey, and their greater enemies the fiſhermen in the different parts of the world.

A Table ſhewing nearly about the place of the grand ſcoole of herrings, and their mean diſtance from the ſun.

* Place and Time.		Latitude.	Longitude.	Sun's Declination.	The Mean Diſtance.
I.	January,	23	70	20 S.	43
II.	February,	32	79	12	44
III.	March,	36	75	0	36
IV.	April,	39	72	10N.	29
V.	May,	49	50	19	30
VI.	June,	65	15	23	42
VII.	July,	58	0	21	37
VIII.	Auguſt,	52	0	14	38
IX.	September,	48	6	0	48
X.	October,	35	22	9	44
XI.	November,	22	40	18	40
XII.	December,	18	52	23	41

* See Map B, in Plate V.

Some Obſervations and Reaſons given for the courſe of the Herrings, and the variation in their Mean Diſtance from the Sun in different months of the year.

[See Map B, Plate V.]

JANUARY. In this month the herrings are ſuppoſed to be returning from too warm a climate and the approaching ſun, from which they retreat faſt.

FEBRUARY. The time of ſpawning now drawing nigh, the herrings, in this month paſs through the gulph ſtream, and fall on the coaſt of America, in order to depoſit their ſpawn in freſh ſhoal water.

MARCH. Now being the beginning of the time of ſpawning, the largeſt and ſtrongeſt fiſh, which perhaps are the oldeſt, ruſh up into the bays, inlets and freſh water ſtreams.

APRIL. In this month the leſſer, weaker, and perhaps younger fiſh, ruſh up even to the heads of ſmall ſtreams, as far as it is poſſible for them to get, and lay their ſpawn. Theſe are twice as numerous as the other.

MAY. Having been detained by the ſpawning ſeaſon, they are overtaken by the ſun, and nearer to it now than at any other time; they therefore haſten out of the rivers in this month, and make great way towards the North ſea.

JUNE. Now having by a rapid progreſs puſhed into a cold climate, on a chilly, icy coaſt, and the ſun beginning to draw towards the ſouth, they whirl round eaſtward.

JULY. The coldneſs of this ſea, and the ſun's declination towards the ſouth, now inclines them that way, in which they fall on the Orkneys, and the ſcoole divides.

AUGUST. The grand ſcoole being divided, now ſurround the whole iſland of Great-Britain and Ireland, and are caught on every ſide.

SEPTEMBER. Having been detained the laſt month by their obſtruction amongſt the iſlands, and being harraſſed by the fiſhermen, their mean diſtance is now the greateſt; they collect into one body and haſten to the ſouthward.

OCTOBER. Being now under great way, they leſſen their mean diſtance, and by the courſe which they ſteer, which perhaps is inclined more weſtward by the current of the trade wind, they paſs the Atlantic.

NOVEMBER. Being now more in the trade, and having approached a warmer climate, their motion is ſuppoſed to incline more weſtward.

DECEMBER. The ſun now beginning to return, they are ſuppoſed to incline more northward, to the place where we began; where they are ſuppoſed to meet their young fry.

N° XXXI.

Obſervations on a Solar and a Lunar Eclipſe, communicated to the Society by M. M. De GRAUCHAIN, *Major General of the French Squadron.*

(Tranſlated from the French.)

GENTLEMEN, Newport, 5th December, 1780.

THE ſtudy of aſtronomy having often occupied my leiſure during the peace, I could not refuſe myſelf even in the midſt of the preparations for war, an oppor-

tunity which preſented of making two important obſervations, which I have the honour of ſending you.

Eclipſes form the baſis of chronology; this may one day ſerve to fix the epocha of the independence of America, one of the moſt intereſting in the hiſtory of mankind. This is a motive to dedicate theſe obſervations to you; and I pay this reſpect with the greateſt pleaſure to an illuſtrious ſociety, whoſe members know how at the ſame time to enlighten their country by their knowledge in mathematics and philoſophy, and to ſerve them ſucceſsfully in their councils and armies.

I am, &c.

DE GRAUCHAIN, Major General of the French Squadron.

MESSIEURS,

L'ETUDE de l'aſtronomie ayant ſouvent occupé mon loiſir pendant la paix, je n'ai pu me refuſer, même au milieu de l'appareil de la guerre, a l'occaſion qui ſut preſentée de faire deux obſervations importantes et j'ai l'honneur de vous les adreſſer. Les eclipſes forment la baſe de la chronologie, et celles cy pourront un jour ſervir a fixir l'epoque de l'independance de l'Amerique l'une des plus interreſſantes de l'hiſtoire du genre humain. C'eſt un motif pour vous en dedier les obſervations, meſſieurs, et je reuds cet hommage avec le plus grand plaiſir a une ſocieté illuſtre dont les membres ſcavant en même temps eclaires leur patrie par leur connoiſances dans les mathematiques et dans la phiſique, et la ſervir utilement dans les conſeils et dans les armées.

Je ſuis avec reſpect, Meſſieurs,

Votre tres humble et tres obeiſſant ſerviteur,

DE GRAUCHAIN, Major general de l'eſcadre Francoiſe.

A Newport le 5 Novembre, 1780.

An

An Obſervation of an Eclipſe of the Sun on the 27th of October, 1780, at Newport in the State of Rhode-Iſland.

	Time by the Clock.			True Time.		
	h.	′	″	h.	′	″
The time that the eclipſe was perceived to begin,	9	24	32	11	0	12*
The preceding limb of the ſun at the vertical,	11	21	39	0	57	27
The upper edge of the ſun at the horizontal,		21	54		57	42
The upper horn of the moon at the horizontal,		22	3		57	51
The edge of the moon at the vertical,		22	45		58	33
The upper horn at the vertical,		23	7		58	55
The lower horn at the vertical,		23	35		59	23†
The lower horn at the horizontal,		29	31	1	5	20
The lower limb of the ſun at the horizontal,		31	2		6	51‡
The preceding limb of the ſun at the vertical,	11	37	12	1	13	1
The upper limb ditto at the horizontal,		37	59		13	48
The upper horn ☽ at the horizontal,		38	52		14	21
The limb ditto at the vertical,	not obſerved.					
The upper horn ditto at the vertical,		38	57		14	16
The lower horn ditto at the vertical,		39	19		15	8
The lower horn ditto at the horizontal,		43	38		19	8
The lower limb of the ſun at the horizontal,		45	27		21	17
The preceding limb ⊙ at the vertical,	11	47	8	1	22	58
The upper edge of ditto at the horizontal,		48	17		24	7
The limb of the ☽ at the vertical,		49	0		24	50
The upper horn at the vertical,		49	5		24	55
The upper horn at the horizontal,		47	7		24	57
The lower limb of ⊙ at the horizontal,		55	2		30	52
The end of the Eclipſe,	12	4	50	1	40	41
The rate of the clock,	At Noon.					
October 21,	10	35	12,8			
24,	10	29	42,0			
25,	10	27	52,3			
27,	10	24	15,8			

* When the ſun was perceived to be indented, it was about 1′ 20″ after the eclipſe began; therefore the true time of beginning was at 10h. 58′ 52″.

† Uncertain.

‡ The ſuperior limb is called the inferior, &c. as the glaſs of the quadrant inverted the objects.

The latitude of the place of obſervation on Goat-Iſland, 41° 30′ 20″ N.

An

An Obſervation of the Eclipſe of the Moon on the 11th of Nov. 1780, at Newport in the State of Rhode-Iſland.

	Time by the Clock.			True Time.		
	h.	′	″	h.	′	″
The beginning of the Eclipſe,	7	40	5	10	24	39
Immerſion of Grimaldi begins,	7	48	50	10	33	25
Ditto, ends,	7	51	25	10	36	10
Immerſion of Tycho begins,	8	0	36	10	45	12
Ditto, ends,	not obſerved.					
The ſhadow to Gallileo,	8	3	42	10	48	18
Immerſion of Copernicus begins,	8	27	54	11	12	31
Ditto, ends,	8	32	35	11	17	12
The ſhadow in the middle of Dionyſius,	8	46	55	11	31	33
The ſhadow of the Pointed Promontary,	8	55	42	11	40	21
Copernicus begins to appear,	9	17	54	12	2	34
Grimaldi - - ditto,	9	22	2	12	6	42
Copernicus wholly appears,	9	23	35	12	8	15
Grimaldi - ditto,	9	26	45	12	11	26
The Pointed Promontary appears,	9	51	12	12	35	55
Tycho wholly appears,	10	10	6	12	54	51
The end of the Eclipſe,	10	32	10	13	16	57
	At Noon.					
The rate of the Clock, November 11,	9	16	15,7			
12,	9	11	30,7			

Remarks upon the Obſervation of the Eclipſe of the Sun.

THE clock by which the time was obſerved, is a pendulum one with a verge of compenſation, made by Mr. Barthond, a celebrated clock maker at Paris. It was regulated many days before and after the obſervation by correſponding altitudes taken with a quadrant of Ramſden, having a radius of one Engliſh foot; it is a very good one and well graduated.

The ſame quadrant ſerved to obſerve the time when the horns and limbs of the moon and ſun arrived at the hori-

Eclairciſſements ſur l'obſervation de l'eclipſe de Soleil.

LA pendule dont on s'eſt ſervi pour obtenir l'heure eſt une pendule a verge de compenſation faite par M. Barthond celebre horloger de Paris, elle a eté reglée pluſieurs jours avant et apres l'obſervation par des hauteurs correſpondantes priſes avec un quart de circle de Ramſden d'un pied anglois de rayon tres bon, et tres bien diviſé.

Le même quart de cercle a ſervi pour les obſervations des paſſages des cornes et des bords du ſoleil et de la lune au fil horiſontal, et au fil vertical de la lunette qui y eſt adaptée.

L'obſervateur qui en embarquant des inſtruments d'aſtronomie n'avoit eu pour objet que de regler des montres marines, n'etoit pas auſſi bien pourvu de lunettes que de pendules et de quart de

horizontal and vertical threads of the glaſs which is fitted to the quadrant.

The obſerver who provided and ſhipped the inſtruments, had no other object in view but to rectify the clock belonging to the ſhip, which was the cauſe that he was not ſo well provided with teleſcopes as with clocks and quadrants, he was therefore obliged to make uſe of a ſimple achromatic ſea-glaſs of four feet focus, to obſerve the beginning and end of the eclipſe.

Yet he believes he can anſwer for the end of the eclipſe within about four or five ſeconds. The inſtant of time which it began is much more uncertain. The ſun was already indented when it was firſt perceived, but in order that he might eſtimate groſsly the true time of its beginning, he has eſtimated pretty nearly the diſtance of the horns, the moment when the ſun's limb was firſt perceived to be indented. By comparing the time elapſed after the end of the eclipſe to the inſtant when the diſtance of the horns of the moon were ſenſibly the ſame, hence he judged that it ſhould have been about 1′ 20″ from the true time in which the eclipſe began until the obſerved time.

At

de cercle, et il a eté obligé de ſe ſervir pour les obſervations du commencement et de la fin de l'eclipſe d'une ſimple lunette achromatic de mer de quatre pieds de foyer.

Cependant on croit pouvoir repondre de la fin de l'eclipſe a quatre ou cinq ſecondes pres; l'inſtant du commencement eſt beaucoup plus incertain; le ſoleil etoit deja conſiderablement entamé, lors qu'on s'en eſt apperçu: pour conclure au moins groſſierement l'inſtant vrai du commencement de l'eclipſe, on a eſtime a peu pres quelle etoit la diſtance des cornes au moment ou on s'eſt apperçû que le diſque du ſoleil etoit entamé, et vers la fin de l'eclipſe on a examiné combien il s'eſt ecoulé de tems depuis l'inſtant ou la diſtance des cornes a eté ſenſiblement la même juſqu'a la fin de l'eclipſe. C'eſt de cette maniere que l'on a jugé qu'il devoit s'etre ecoulé environ 1′ 20″ depuis l'inſtant vrai du commencement de l'eclipſe, juſqûà celui ou on s'eſt apperçû, quelle etoit commencée.

On a dabord crû inutile de chercher a obſerver la grandeur de l'eclipſe avec le quart de cercle, a cauſe de la lenteur du movement des deux aſtres dans le ſens vertical, cependant a la reflexion on a penſé que les paſſages des cornes et des bords du ſoleil et de la lune au fil vertical ſeulement ſuffiroient pour donner les differences de hauteur et d'azimuth des centres des deux aſtres, et par conſequent leur difference de latitude et de longitude. On les a donc obſervês vers la fin de l'eclipſe, et en même tems on a obſervé les paſſages au fil horiſontal, mais ſans eſperer quils puſſent etre d'un grand ſecours pour calculer la diſtance des centres.

Dans la premiere obſervation le paſſage de la corne ſuperieure au fil vertical eſt un peu douteux. On penſe donc qu'il eſt a propos d'employer de preference dans le calcul de cette obſervation les paſſages du bord de la lune, et de la corne inferieure au même fil vertical. De cette maniere on connoitra immediatement les lignes N B et C O dont la premiere combinée avec les deux diametres L N et S T donnera L E; difference d'azimuth des centres des deux aſtres. On obtiendra auſſi facilement la difference apparente de hauteur S E des mêmes centres en calculant

At firſt it was thought uſeleſs to endeavour to obſerve the magnitude of the eclipſe with the quadrant, becauſe of the ſlow movement of the two planets vertically, yet upon reflection he thought that the paſſage of the horns and limb of the ſun and moon to the vertical thread only, would be ſufficient to give the differencies of the altitude and azimuth of the centers of the ſun and moon, and of courſe their difference of latitude and longitude. He then obſerved them towards the end of the eclipſe, and at the ſame time obſerved the time of their arrival at the horizontal thread, but without any expectation of their being of great ſervice to calculate the diſtance of the centers from.

In the firſt obſervation the paſſage of the upper horn to the vertical thread, is a little doubtful; at the time it was thought proper to give the preference to the paſſages of the limb and lower horn of the moon to the ſame vertical thread, in making the calculations from this obſervation; by this means we may find the lines NB and CO, the firſt of which combined with the two diameters LN and ST will give LE, the difference of azimuths and of the centers of the two planets; the apparent difference of the altitude from the ſame centers may be eaſily obtained, by calculating SF and CM, ſides of the right-angled triangles CSF and CLM, in which are known the other two ſides.

Plate III. Figure 18.

In

culant SF et CM cotes des triangles rectangles CSF et CLM dans les quels on connoit deja les deux autres cotés.

Dans la ſeconde obſervation on a obmis par diſtraction, l'inſtant du paſſage du bord de la lune au fil vertical, on ſe ſervira donc pour la calculer des paſſages des cornes au même fil les quels donneront immediatement CA et CO d'ou ſouſtrayant ST, on aura CD et CF connoiſſant ces deux lignes et le demi-diametre du ſoleil on calculera les angles CSD, CSF ſouſtrayant leur ſomme de 180°, on aura l'angle CSL partageant cet angle par la moitié on aura l'angle CSL du triangle CLS, on connoitra donc aiſement SL coté de cetriangle et du triangle SLE calculant enfin ce dernier triangle SLE dans le quel on connoit deux angles et un côtè on obtiendra LE; difference d'azumuth et SE; difference apparente de hauteur des centres des deux aſtres.

La derniere obſervation etant plus complette on pourra la calculer indifferement de l'une ou l'autre maniere. On pourra même faire uſage du paſſage au fil horiſontal pour conclure la difference de hauteur attendu que le movement des deux aſtres dans le ſens vertical commencoit a devenir moins lent lorſque cette obſervation a eté faite.

In the ſecond obſervation was omitted, by inattention, the inſtant of the arrival of the limb of the moon to the vertical thread, which however may be calculated by the arrival of the horns to the ſame thread, by which CA and CO are obtained: By ſubtracting ST from theſe, the remainder will be CD and CF, having theſe two lines and the ſemi-diameter of the ſun, the angles CSD, CSF, may be had, and ſubtracting their ſum from 180° the remainder will be the angle CSC, the half of which is the angle CSL; from the triangle CSL you may readily obtain SL, a ſide of this triangle, and of the triangle SLE; from theſe at length this laſt triangle SLE may be calculated, in which are had two angles and one ſide, from which may be obtained LE the difference of azimuth, and SE the apparent difference of altitude of the centers of the ſun and moon.

My laſt obſervation being more complete, might be calculated either from the one or the other method, and the paſſage to the horizontal thread might even be uſed to determine the difference of the altitude of the centers, as the motion of theſe planets with reſpect to the vertical, began to be quicker when this obſervation was made.

If we are deſirous to aſcertain the time of the paſſage of the upper horn of the moon to the vertical thread in the firſt obſervation, or to know in the ſecond obſervation the time of the paſſage of the limb of the moon by the ſame thread, they may eaſily be calculated by the help of the quantities already found. Perhaps it might be uſeful to make this calculation, to determine the variation which ought to have place in the poſition of the two points obſerved,

Si l'on vouloit dans la premiere obſervation veriffier l'inſtant du paſſage de la corne ſuperieure au fil vertical, ou connoitre dans la ſeconde obſervation l'inſtant du paſſage du bord de la lune au même fil on pourroit aiſement les calculer avec le ſocours des quantites deja connues. Il ſera peutetre même utile de faire ce calcul pour ſe mettre en etat de determiner la variation qui a du avoir lieu dans la poſition des deux points obſervér relativement l'un a l'autre, pendant l'eſpace de tems ecoulé entre leurs paſſages au même fil.

relatively to each other during the time elapſed between their paſſage to the ſame thread.

Obſervation de l'èclipſe de Lune.

ON a fait uſage dans cette obſervation de la même quart de cercle, et de la même lunette qui avoient ſervi pour l'obſervation de l'èclipſe de ſoleil. La marche de la pendule etoit cependant un peu differente parce qu'on y avoit touché.

N° XXXII.

An Account of the Tranſit of Venus over the Sun, June 3d, 1769, as obſerved at Newbury, in Maſſachuſetts; by the Rev. SAMUEL WILLIAMS, A. M.

THE tranſit of Venus over the ſun, being one of the moſt uncommon and uſeful phenomena in aſtronomy, I determined to make as careful an obſervation of it as I could. Early in May I received an invitation from *Triſtram Dalton*, Eſq. a gentleman of Newbury-Port, to obſerve it with him. He had a ſeat at Newbury, in a high elevated ſituation, very convenient for this purpoſe, at which we agreed to make the obſervation. The weather for ſeveral days had been dull and rainy, but clearing up on Tueſday evening I went early on Wedneſday to put every thing in readineſs. The regulation of our clock being an article of great importance, I was very careful to have it thoroughly examined, and well fitted up. To adjuſt it to apparent time we took correſponding altitudes of the ſun, both before and on the day of the tranſit. In theſe obſervations, it was eaſy to arrive to a pretty great exactneſs; and as they were very numerous, the going of the clock was well aſcertained by them, and found to be ſteady and regular. The teleſcope we had prepared was a reflector made by *Nairne*, magnifying about 55 times; a good inſtrument, but not fitted with a micrometer,

micrometer, or with vertical and horizontal hairs, as we could have wiſhed.

The third of June proved favourable to our wiſhes. The air was uncommonly clear, and the ſky ſerene. About twenty minutes before the tranſit, I began to keep my eye ſteadily fixed on that part of the ſun's limb, on which the planet by calculation was to enter; an aſſiſtant counting the clock in the mean time, while another ſtood by to write down the obſervations. Thus prepared, we waited with a kind of agreeable anxiety for the high ſatisfaction of ſeeing Venus on the ſun; a ſatisfaction I had once before enjoyed in viewing the tranſit of 1761*, and which I knew muſt end with that of 1769! The firſt impreſſion of Venus on the ſun, I expected would not appear like a diſtinct well defined black ſpot coming on as it were in an inſtant, but rather like an ill defined mixture of limbs. The event was agreeable to the conjecture, for at 2^h $30'$ $14''$, apparent time, I imagined I ſaw *a ſmall diſturbance* on the ſun's limb; but the impreſſion was then ſo ſmall, irregular and ill defined, that it was not till after ſeveral ſeconds that I was certain the tranſit was begun. But the impreſſion increaſing and growing more diſtinct, I fixed on the time mentioned above as the time of the *external contact*. To obſervers with teleſcopes and eyes equally good, and fixed on that part of the ſun on which the planet entered, I conceive this firſt impreſſion might have been obſerved to an agreement of 5 or 6 ſeconds. Though perhaps it might be the contact of the atmoſphere, rather than of the body of Venus with the ſun.

In about ten minutes after the *external*, I began to look for the *internal contact*. From the form in which Venus appeared, being ſurrounded with a glimmering light, not very diſtinctly defined, I concluded it would be difficult if not impoſſible to fix upon the preciſe moment when her

* At St. *John's*, in *Newfoundland*.

limb would be exactly coincident with that of the ſun; and therefore determined to wait till there ſhould appear a ſmall thread of light between them. As the contact drew near, the thread of light began to form, and ſeemed to dart on each ſide of the planet for ſeveral ſeconds without being fixed or ſettled. At $2^h\ 48'\ 44''$, with a ſeeming uncertainty of not more than $7''$ it became cloſed and fixed; Venus then appeared wholly within the ſun, ſeparated from its limb by a fine ſtream of light flowing gently round it. This I fixed upon as the *internal contact*, though this might alſo be the contact not of Venus but of her atmoſphere with the ſun. Not having a *micrometer* or *hairs* fixed in the reflector, inſtead of making any further obſervations, we could only enjoy the pleaſure of viewing this curious phenomenon, and ſhowing it to a number of gentlemen that had aſſembled on the occaſion.

To determine the latitude of the place, we took the meridian altitude of the ſun on the day of the tranſit, by tranſmitting his rays from a ſtyle 10 feet high, upon a large horizontal platform. From this obſervation our latitude came out $43°\ 2'$ north. Suſpecting the obſervation was not ſufficiently accurate, I have ſince carefully examined the matter, and from ſeveral obſervations which nearly agree, I find it to be but $42°\ 57'$. With regard to our longitude, the mean of ſix or ſeven obſervations of the eclipſes of Jupiter's firſt ſatellite, gives it about $4^h\ 42'\ 30''$ weſt from *Greenwich*.

In the above account of the *contacts*, the duration of the ingreſs, or paſſage of Venus over the ſun's limb, is $18'\ 30''$; near a minute longer than in moſt of the *American* obſervations. By theory it ſhould be $18'\ 56''$, but as this muſt have been contracted at the place of obſervation, $15''$, by parallax, the apparent duration of the ingreſs, would be but $18'\ 41''$; that is, $11''$ longer than it was made by obſervation. I much doubt whether it was poſſible to diſcern the planet ſo ſoon as $11''$ after the firſt

firſt contact, when not a ſecond of its diameter had entered upon the ſun. It is moſt probable that the *internal contact* was paſt before the thread of light appeared to me to be compleated. It ſeems as though ſomething of the ſame kind, muſt alſo have been the caſe in moſt of the *European* obſervations; as they make the ingreſs near a minute longer, than it was ſeen by moſt of the *American* obſervers, when by theory it muſt rather have been ſhorter. But the different appearances of Venus, different ideas of the contacts, with the unavoidable difference of eyes, teleſcopes, the ſtate of the atmoſphere and the like, might eaſily occaſion ſuch differences in the obſervations. Though in the ſame circumſtances, it can hardly be thought but that the *European* and the *American* obſervations would have more nearly agreed.

An Account of the Tranſit of Mercury over the Sun, November 9th, 1769, as obſerved at Salem, in Maſſachuſetts; by the Rev. SAMUEL WILLIAMS, A. M.

THE tranſits of Mercury, though they are not of equal uſe in aſtronomy with thoſe of Venus, are yet of great advantage to perfect the elements of his theory, and to determine the longitude of places on the earth. I had an opportunity to obſerve one of theſe tranſits, November 9, 1769, in company with *Andrew Oliver*, Eſq. at *Salem*. Mr. Oliver had a good reflector, magnifying about ſixty times. But his clock not being in ſo good order as was to be wiſhed, and not having any inſtrument to take altitudes, I was obliged to have recourſe to the following method to determine the time. The day before the tranſit I drew a meridian line, with which I examined the going of the town clock on the day of the tranſit, and on the day after, and found it had kept time very well. Comparing my watch with the clock, the time was pointed out to minutes pretty exactly. Taking the minutes from

from the watch, I endeavoured to count the ſeconds, which by a perſon uſed to it may be done pretty near the truth. This method of determining the time, though ſuch as an aſtronomer would by no means chuſe, was the only one that I could make uſe of; and from the pains I took to be exact, I believe it might be depended upon to eight or ten ſeconds.

At the *firſt contact* I expected *Mercury* would have appeared as Venus had done, ſomething irregular, uneven, and not very diſtinctly defined. But at 2^h $54'$ $40''$ apparent time, I was agreeably diſappointed by ſeeing the planet come on as it were in an inſtant, in the form of a clear, regular, well defined black ſpot. The *internal contact* was equally inſtantaneous; at 2^h $56'$ $0''$ the thread of light cloſed to appearance in a moment, without a ſeeming uncertainty of a ſecond. The ſky being perfectly clear and ſerene, nothing could be better defined than the limbs of Mercury and the ſun. There was no appearance of any thing like an atmoſphere round the planet, but all the time the ſun was viſible, Mercury appeared like a ſteady diſtinct black ſpot, much leſs than ſome that were then upon the ſun. Not having a micrometer, it was not in our power to make any further obſervations, either on the diameter of the ſun or Mercury, or of the leaſt diſtance of their limbs.

An Obſervation of an Eclipſe of the Sun, November 6th, 1771, at Bradford, in Maſſachuſetts; by the Rev. SAMUEL WILLIAMS, A. M.

FROM the beginning of the year 1769 till the end of 1771, there were but two eclipſes that could be obſerved at *Bradford*. One of theſe was a total eclipſe of the moon, June 19th, 1769; of this I had no obſervation, being prevented by an indiſpoſition. The other was an eclipſe of the ſun, November 6th, 1771. The weather for ſeveral

ral days before, was ſo cloudy that I attempted in vain to regulate my clock, though I watched every favourable opportunity. On the day of the eclipſe I got it pretty well adjuſted by ſeveral correſponding altitudes of the ſun. About 1^h P. M. the clouds gathered ſo much round the ſun, that I was apprehenſive they would prevent any obſervation. But being pretty much ſcattered, at $1^h\ 36'\ 42''$ apparent time, I could very plainly perceive that the eclipſe was juſt begun. This I judged was very near the beginning, if not exactly ſo, though it was attended with ſome uncertainty. In a few minutes the ſun was wholly covered with the clouds, and remained thus till $3\frac{1}{2}^h$, when they began again to ſcatter, and left that part of the heavens in which the ſun appeared, perfectly clear. The weather continued thus till the end of the eclipſe, which by a good obſervation was at $3^h\ 47'\ 2''$. Theſe obſervations were made with a reflector made by *Nairne*, magnifying as near as I could judge about ſixty times; but as to the *quantity* of the eclipſe, no obſervation could be made, the ſun being obſcured by the clouds the biggeſt part of the time.

N° XXXIII.

An eaſy and accurate Method of finding a true Meridian Line, and thence the Variation of the Compaſs.

By ROBERT PATTERSON.

Read Apr. 7, 1786.

OF the various methods which aſtronomers employ for finding a true meridian line, none ſeems ſo well adapted, as could be wiſhed, to the common uſe of ſurveyors, in finding the variation of the Compaſs.

To find the azimuth of the ſun by a ſingle obſervation of his altitude, beſides a quadrant which is neceſſary fo-

thi-

this purpoſe, requires the previous knowledge either of the latitude of the place, or hour of the day, at the time of obſervation; neither of which can, by the common apparatus of a ſurveyor, be found with ſufficient accuracy.

The ſun's azimuth may, it is true, be found without knowing either the latitude of the place or hour of the day, by taking equal altitudes before and after noon; but this requires time, attention and inſtruments, which ſurveyors can but ſeldom command.

That method, which is perhaps the moſt exact, viz. meaſuring the time between the paſſage of two ſtars which differ conſiderably, in declination and but little in right aſcenſion, over the ſame vertical circle, is ſtill farther out of the reach of common ſurveyors.

The following table of the pole ſtar will, it is preſumed, furniſh a more eaſy, and yet ſufficiently accurate method of determining this problem; free from all the above inconveniencies, and requiring no difficult calculation, nor any other inſtrument than the common theodolite, or circumferentor. For though the latitude of the place ſhould not be known within a whole degree, nor the hour of the night within 2 or 3 minutes, this table, by a ſingle obſervation of the magnetic azimuth or bearing of the pole ſtar, will generally give the variation of the needle true to a ſingle minute of a degree. Nay if the obſervation be made (as it may be every night) when the ſtar is near its greateſt elongation, an error of 10, or even 20 minutes in time will, as is plain from the table, produce little or no ſenſible error in the azimuth. And as theſe obſervations may be repeated at pleaſure during the night, and a mean of all taken, the variation may, by this means, be found to any degree of accuracy that can be deſired. Beſides, the needle is not at this time affected with any diurnal variation; which in the day-time is very uncertain, and frequently amounts to more than one quarter of a degree.

The

The beſt inſtrument for obſerving the ſtar's magnetic azimuth is a theodolite, furniſhed with ſpirit-levels, and a ſmall teleſcope with a perpendicular wire. A common circumferentor may, however, anſwer the purpoſe. When this inſtrument is uſed, a fine thread or hair muſt be ſtretched along from the top of one ſight to that of the other, directly over the center of the compaſs; and the obſerver muſt be very careful to place the ſights perpendicular to the horizon when he makes the obſervation; for this purpoſe a ſmall pocket ſpirit-level, in the form of a carpenter's ſquare, would be very convenient.

By the common circumferentor we cannot, indeed, take the bearing of an object with very minute accuracy; for though the eye can very well judge of the coincidence of two lines, or of the point of the needle with any whole degree on the compaſs, yet the parts of a degree cannot readily be obſerved to greater exactneſs than one third or one fourth of the whole. This inconvenience may, however, be eaſily remedied, and at a very trifling expence, in the following manner.

Let one of the ſights, by means of a ſcrew, be made movable at right angles to the index; and on the end of the index, cloſe to the movable ſight, ſet off, on each ſide of the central line, the tangent of three degrees to a radius equal to the whole length of the index, or diſtance between the two ſights. Let each of theſe degrees be divided into ſix equal parts; then will a nonius diviſion on the ſight, where ten equal parts muſt correſpond with eleven on the index, ſubdivide theſe parts into minutes of a degree.

It will be unneceſſary to make the ſight move in the arch of a circle, the difference between this and the tangent, in ſo ſmall an arch, being quite imperceptible. With this ſimple improvement the common circumferentor will take the bearing of an object true to a minute, thus: Let the end of the needle be made exactly to coincide with the

neareſt whole degree, then move the ſcrew till the object appears in the direction of the ſights, and the nonius on the movable ſight will point out the odd minutes.

Explanation and Uſe of the TABLE.

The left hand double column of the table contains the time before the ſtar's paſſage over the meridian above the pole, for every twenty minutes of its whole diurnal circuit. The firſt column, under each particular latitude, ſhews the azimuth of the ſtar at theſe times, reſpectively, in degrees, minutes and tenths of a minute. The ſecond column ſhews the difference of azimuth in every twenty minutes of intermediate time, in minutes and tenths.

To find the true azimuth of the ſtar in any latitude, at any given time.

From the ſtar's right aſcenſion, viz. $0^h\ 49^m$, increaſed by 24^h if neceſſary, ſubtract the right aſcenſion of the ſun computed to the time of the ſtar's paſſage over the meridian, above the pole, nearly, the remainder will be the time of ſaid paſſage, reckoned from noon. From which, increaſed by 24^h if neceſſary, ſubtract the time of the obſervation, reckoned alſo from noon, the remainder will ſhew the time before the ſtar comes to the ſaid meridian. Look for this time in the left hand column of the table, oppoſite to which in the column of azimuth, under the proper latitude, you will have the true azimuth of the ſtar at that time.

If the time before the ſtar comes to the meridian be leſs than 12 hours, its azimuth will be eaſterly; but if more than 12 hours, its azimuth will be weſterly.

If the magnetic azimuth, and the true azimuth at the time of the obſervation, be both eaſterly or both weſterly, their difference will be the variation of the needle. But if one be eaſterly and the other weſterly, their ſum will be

be the variation. And if the magnetic be to the weſtward of the true azimuth, the variation will be weſterly; but if to the eaſtward, the variation will be eaſterly.

If the time before the ſtar's paſſage over the meridian be ſome intermediate minute, or the latitude of the place ſome intermediate degree, not found in the table, a proportional intermediate azimuth, by means of the differences, muſt be taken.

The right aſcenſion of the pole ſtar annually increaſes 10 ſeconds of time, and its polar diſtance decreaſes 20 ſeconds of a degree, therefore to its preſent right aſcenſion (in 1785,) viz. $0^h\ 49^m$, muſt be added one minute every year; and from its preſent polar diſtance ($1^\circ\ 50'.5$) one minute muſt be ſubtracted, and a proportional part from all the numbers in the columns of azimuth, every three years. The effect of aberration and nutation may be ſafely neglected; as the error ariſing from theſe cauſes can never amount to more than half a minute of a degree in azimuth.

In computing the ſun's right aſcenſion to the time of the ſtar's paſſage over the meridian nearly, the following little table will be uſeful.

TABLE.

Time.		Star paſſes Meridian nearly at
April	2	Noon
	19	11 A. M.
May	5	10
	20	9
June	4	8
	18	7
July	3	6
	17	5
Auguſt	2	4
	17	3
September	3	2
	19	1
October	6	Midnight
	22	11 P. M.
November	6	10
	21	9
December	5	8
	19	7
January	1	6
	15	5
	29	4
February	13	3
March	1	2
	17	1

EXAMPLE I.

Suppoſe on the 12th of September 1785, at 8 o'clock in the evening, in the latitude of 40° N. the magnetic azimuth of the pole-ſtar had been obſerved to be 0° 38′ eaſterly; required the variation of the needle at the time and place of obſervation.

Star's

	H.	M.
Star's R. A. increaſed by 24 hours, -	24	49
Sun's R. A. computed to 1^h A. M. (taken from the nautical almanac, or any other table of the ſun's R. A.) ſubtract -	11	25
True time of ſtar's paſſage over meridian, reckoned from noon, - -	13	24
Hour of the night ſubtract, - -	8	0
Time before ſtar's paſſage, - -	5	24

Which correſponds to true azimuth,	2° 23′	E.
Magnetic azimuth, - -	0 38	E.
Variation of the needle, -	1 45	W.

EXAMPLE II.

In the latitude of about 32° north, on the 4th of July 1785, at 48 minutes after 10 o'clock at night, ſuppoſe the magnetic azimuth of the pole ſtar, to be 2° 40′ eaſt; required the variation of the needle.

	H.	M.
Star's R. A. + 24^h, - - -	24	49
Sun's R. A. - - - -	6	54
Time of ſtar's paſſing meridian, -	17	55
Time of obſervation, - - -	10	48
Time before ſtar comes to meridian, -	7	7

Which correſponds to true azimuth,	2° 4′	E.
Magnetic azimuth, - -	2 40	E.
Variation, - -	0 38	E.

Ex-

EXAMPLE III.

Latitude of the place 42° north, time of observation, January 17th 1785, at 2^h 40^m A. M. Magnetic azimuth, 1° 5′ easterly.

	H.	M.
Star's R. A. + 24^h - - -	24	49
Sun's R. A. - - - -	20	1
Time of star's passing the meridian, -	4	48
	24	
	28	48
Time of observation reckoned from noon,	14	40
Time before star comes to meridian, -	14	8

Corresponding to true azimuth,	1° 16′	W.
Magnetic azimuth, - -	1 5	E.
Variation, - - -	2 21	E.

A TABLE

A TABLE of the Azimuth of the Pole-ſtar for every 20 Minutes of its diurnal Motion round the Pole.

Time before the ſtar comes to the meridian above the pole.		Latitude 30°		Latitude 35°		Latitude 40°		Latitude 45°		Latitude 50°		Latitude 55°	
H. M.	H. M.	Star's azimuth.	Diff.	Star's azimuth.	Diff.	Star's azimuth.	Diff.	Star's azimuth.	Diff.	Star's azimuth.	Diff.	Star's azimuth.	Diff.
		° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
0 0	24 0	0 0.0		0 0.0		0 0.0		0 0.0		0 0.0		0 0.0	
			11.3		12.0		12.9		14.1		15.6		17.6
20	23 40	11.3	11.3	12.0	12.0	12.9	12.8	14.1	13.9	15.6	15.4	17.6	17.4
40	20	22.6		24.0		25.7		28.0		31.0		35.0	
			11.1		11.7		12.6		13.7		15.2		17.2
1 0	0	33.7		35.7		38.3		41.7		46.2		52.2	
			10.7		11.5		12.3		13.4		14.8		16.7
20	22 40	44.4	10.4	47.2	11.0	50.6	11.9	55.1	12.9	1 1.0	14.3	1 8.9	16.0
40	20	54.8		58.2		1 2.5		1 8.0		15.3		24.9	
			10.0		10.6		11.3		12.3		13.6		15.4
2 0	0	1 4.8		1 8.8		13.8		20.3		28.9		40.3	
			9.5		10.0		10.9		11.8		12.9		14.5
20	21 40	14.3	8.9	18.8	9.4	24.7	10.0	32.1	10.9	41.8	12.0	54.8	13.5
40	20	23.2		28.2		34.7		43.0		53.8		2 8.3	
			8.2		8.8		9.3		10.1		11.2		12.5
3 0	0	31.4		37.0		44.0		53.1		2 5.0		20.8	
			7.6		7.8		8.5		9.1		10.0		11.2
20	20 40	39.0	6.7	44.8	7.1	52.5	7.5	2 2.2	8.2	15.0	9.0	32.0	10.0
40	20	45.7		51.9		2 0.0		10.4		24.0		42.0	
			5.9		6.3		6.7		7.2		7.7		8.7
4 0	0	51.6		58.2		6.7		17.6		31.7		50.7	
			5.0		5.2		5.5		6.0		6.6		7.3
20	19 40	56.6	4.1	2 3.4	4.3	12.2	4.5	23.6	4.9	38.3	5.4	58.0	6.6
40	20	2 0.7		7.7		16.7		28.5		43.7		3 4.6	
			3.1		3.3		3.6		3.7		4.0		3.6
5 0	0	3.8		11.0		20.3		32.2		47.7		8.2	
			2.2		2.3		2.4		2.5		2.6		3.0
20	18 40	6.0	1.3	13.3	1.4	22.7	1.3	34.7	1.4	50.3	1.4	11.2	1.4
40	20	7.3		14.7		24.0		36.1		51.7		12.6	
			.7		.6		.9		1.0		1.1		1.5
6 0	0	7.6		14.8		24.2		36.2		51.8		12.5	
			1.7		1.9		2.0		2.2		2.5		2.9
20	17 40	6.9	2.5	14.2	2.8	23.3	3.0	35.2	3.3	50.7	3.9	11.0	4.3
40	20	5.2		12.3		21.3		33.0		48.2		8.1	
			3.5		3.8		4.1		4.5		4.9		5.6
7 0	0	2.7		9.5		18.3		29.7		44.3		3.8	
			4.5		4.6		5.0		5.5		6.2		7.0
20	16 40	1 59.2		5.7		14.2		25.2		39.4		2 58.2	
40	20	54.7		1.1		9.2		19.7		33.2		51.2	
			5.1		5.6		6.0		6.5		7.1		8.2
8 0	0	49.6		1 55.5		3.2		13.2		26.1		43.0	
			6.2		6.4		6.9		7.5		8.4		9.3
20	15 40	43.4	6.8	49.1	7.3	1 56.3	7.6	5.7	8.5	17.7	9.2	33.7	10.4
40	20	36.6		41.8		48.7		1 57.2		8.5		23.3	
			7.5		7.9		8.5		9.2		10.2		11.5
9 0	0	29.1		33.9		40.2		48.0		1 58.3		11.8	
			8.2		8.7		9.4		9.9		11.0		12.1
20	14 40	20.9	8.8	25.2	9.2	30.8	9.9	38.1	10.8	47.3	11.7	1 59.7	13.2
40	20	12.1		16.0		20.9		27.3		35.6		46.5	
			9.3		9.8		10.4		11.3		12.4		13.8
10 0	0	2.8		6.2		10.5		16.0		23.2		32.7	
			9.7		10.3		11.0		11.8		12.9		14.5
20	13 40	0 53.1	10.2	0 55.9	10.7	0 59.5	11.4	4.2	12.3	10.3	13.4	18.2	14.9
40	20	42.9		45.2		48.1		0 51.9		0 56.9		3.3	
			10.5		11.0		11.7		12.7		14.0		15.6
11 0	0	32.4		34.2		36.4		39.2		42.9		0 47.7	
			10.7		11.3		12.0		12.9		14.2		15.7
20	12 40	21.7	10.8	22.9	11.4	24.4	12.2	26.3	13.1	28.7	14.3	32.0	15.9
40	20	10.9	10.9	11.5	11.5	12.2	12.2	13.2	13.2	14.4	14.4	16.1	16.1
12 0	0	0.0		0.0		0.0		0.0		0.0		0.0	

N° XXXIV.

Aſtronomical Obſervations, communicated by Mr. RITTENHOUSE.

By Mr. *James Six*, of Canterbury.

Geocentric place of the New Planet.

April 1ſt, 1782,	29°	5′	30″	in ♊ lat. 13′ N.
October 15th,	7	21	18	♋ nearly ſtationary.
December 26th,	5	2	30	in oppoſition.
March 10th, 1783,	3	15	0	ſtationary.
October 15th,	11	53	10	ſtationary.
December 30th,	9	47	25	in oppoſition.
March 14th, 1784,	7	46	0	ſtationary.

Lat. 23 N.

By *D. Rittenhouſe*, at Philadelphia.

Tranſit of Mercury over the ſun's diſk, Nov. 12th, 1782.

Firſt external contact,	9h 34′ 50″ morn.	Mean Time.
Internal, uncertain,	40 0	
Second internal,	10 51 30	
Laſt contact,	57 35	

Greateſt diſtance of ☿ center from ſun's limb, 31″.

1784,		On Meridian.
Jan. 29th.	γ Gemino	9h 49′ 20″
	ε Gem.	9 54 45
	New Planet	10 1 48
	ζ Gem.	10 15 19
Feb. 12th.	γ Gem.	8h 54′ 19″
	New Planet,	9 4 54
	ζ Gem.	9 20 18

1785.		On Meridian.		
January 26th.	ζ Gemino	10h	24′	14″
	New Planet	10	31	22
	δ Gem.	10	40	7
February 4th.	ζ Gem.	9	48	48
	New Planet	9	54	32
	δ Gem.	10	4	41

Feb. 12th. Obſerved zenith diſtance, { ζ Gemino 19° 5′ 7″; New Planet 16 40 45; δ Gem. 17 35 33 }

Febr. 17th.	ζ Gem.	8h	57′	42″
	New Planet	9	1	46
	δ Gem.	9	13	35

zenith diſtance, 16° 40′ 42″
ditto, 17 35 40

Febr. 27th.	ζ Gem.	8	18	34
	New Planet	8	21	32
	δ Gem.	8	34	27
March 12th.	ζ Gem.	7	27	14
	New Planet	7	29	41½
	δ Gem.	7	43	6½
March 15th.	Sirius	6	59	51
	ζ Gemino	7	15	30
	New Planet	7	17	54
	δ Gem.	7	31	23
March 17th.	Sirius	6	51	58
	ζ Gem.	7	7	37
	New Planet	7	9	59
March 22d.	Sirius	6	32	18
	ζ Gem.	6	47	56
	New Planet	6	50	19
March 27th.	Sirius	6	12	38
	ζ Gem.	6	28	16
	New Planet	6	30	45
28th.	ζ Gem.	6	24	19
	New Planet	6	26	50½

1786,					Micrometer meaſure of Z. D.		Difference in min. & ſec.
January 25th.	δ Gemino	10h	45′	1″			
	New Planet	10	56	48			
					r.	d.	
26th.	δ Gem.	10	41	5	+	1 35	
	New Planet	10	52	44	—	4 37½	

Difference, 6 24½ = 20′ 59″

1786,		On Meridian.			Micrometer measure of Z. D. r. d.		Difference in min. & secs.
January 27th.	♂ Gemino	10h	37′	8″	+	1 34	
	New Planet	10	43	37	—	4 34	
					Diff.	6 29	= 21′ 17″
31st.	♂ Gem.	10	21	24			
	New Planet	10	32	13			
February 1st.	♂ Gem.	10	17	28	Diff.	7 0	= 22 33
	Pluto	10	28	7			
2d.	μ Gem.	9	16	23½			
	♂ Gem.	10	13	33		8 40	
	New Planet	10	24	2		16 0	
					Diff.	7 8	= 23 5
3d.	♂ Gem. - - - -					8 39	
	New Planet - - -					16 3½	
					Diff.	7 12½	= 23 24
6th.	♂ Gem.	9	57	50		8 39¾	
	New Planet	10	7	43		16 17	
					Diff.	7 25¼	= 24 7
10th.	μ Gem. - - - -					13 21½	
	♂ Gem.	9	42	7			
	New Planet	9	51	24		16 29½	
					Diff.	3 8	= 10 13
21st.	♂ Gem.	8	58	52			
	New Planet	9	6	46			
March 10th.	♂ Gem.	7	52	2		12 5½	
	New Planet	7	58	31		21 21	
					Diff.	9 15½	= 30 8
11th.	μ Gem.	6	50	55			
	♂ Gem.	7	48	5		12 5	
	New Planet	7	54	31		21 20¾	
					Diff.	9 15¾	= 30 4
12th.	♂ Gem.	7	44	9		13 17½	
	New Planet	7	50	32		22 34½	
					Diff.	9 17	= 30 9
18th.	μ Gem.	6	23	23			
	♂ Gem.	7	20	33		13 16¾	
	New Planet	7	26	43½		22 35½	
					Diff.	9 18¾	= 30 16

1786,		On Meridian.			Micrometer measure of Z. D. r. d.		Difference in min. & seco.
March 19th.	δ Gem.	7	16	37½	14	36	
	New Planet	7	22	47½	24	8¾	
					Diff. 9	20¾	= 30 24
21ſt.	δ Gem.	7	8	46	5	18	
	New Planet	7	14	53	14	37¼	
					Diff. 9	19¼	= 30 18
23d.	δ Gem.	7	0	54½	3	28½	
	New Planet	7	7	1½	12	47	
					Diff. 9	18½	= 30 15
24th.	δ Gem.	6	56	59	8	14¾	
	New Planet	7	3	6	17	34	
					Diff. 9	19¼	= 30 18
25th.	δ Gem.	6	53	3			
	New Planet	6	59	16			
27th.	δ Gem.	6	45	11	8	13¾	
	New Planet	6	51	18	17	31	
					Diff. 9	17¼	= 30 10
April 2d.	δ Gem.	6	21	36			
	New Planet	6	27	53			

N. B. In theſe obſervations the declination of the New Planet was conſtantly greater than that of δ Geminorum, but leſs than μ.

N° XXXV.

A Letter from Mr. OTTO, *to* Dr. FRANKLIN, *with a Memoir on the Diſcovery of America.*

New-York, 1ſt April, 1786.

SIR,

Read Apr. 7, 1786.

ALMOST all the authors who have written upon the diſcovery of America, make mention of ſome information which Chriſtopher Columbus procured at Madeira, upon the exiſtence of a weſtern continent; but they do not tell us, poſitively, how far this in-

formation assisted him, or from what source he derived it. I have always been curious to clear up this interesting part of history; and in running over many ancient historians, as well German as Spanish, I have found some circumstances, which have appeared to me to establish, in the clearest manner, a discovery anterior to that of Columbus. I have the honor to send you the result of my enquiries; and if you think this piece worthy of being submitted to the consideration of the Philosophical Society, I beg you to present it to them as a mark of my homage, and of the desire which I have of being of some service.

I have the honor to be, with respectful attachment,

Your excellency's very humble and

Most obedient servant,

OTTO.

His excellency Dr. Franklin.

A New-York, le 1 Avril, 1786.

MONSIEUR,

PRESQUE tous les auteurs qui ont ecrit sur la decouverte de l'Amerique, font mention de quelques renseignemens que Christophe Colomb s'est procurés dans l'isle de Madere sur l'existence d'un continent occidental, mais ils ne nous disent pas positivement jusqu' à quel point ces relations ont pu lui être utiles, ou quelle en a été la source. J'ai toujours été curieux de debrouiller cette partie intéressante de l'histoire; et en parcourant plusieurs anciens historiens, tant Allemans qu'Espagnols, j'ai trouvé quelques détails qui m'ont paru établir d'une maniere indubitable une decouverte anterieure à celle de Colomb. J'ai l'honneur de vous en adresser le resumé, et si vous croyés que cette piece soit digne d'être mise sous les yeux de la societé philosophique, je vous supplie de la lui presenter comme une marque de mon hommage et du desir que j'ai de lui être de quelqué utilité.

J'ai l'honneur d'être avec un respectueux attachement,

MONSIEUR,

De votre Excellence,

Le très humble et très obeissant serviteur,

OTTO.

S. E. M. FRANKLIN.

Memoir upon the Diſcovery of America.

IT has always been looked on as a piece of injuſtice, not to have given the name of Columbus to that valuable part of the world which he diſcovered; and that Americanus Veſpucius, who did nothing but follow his footſteps, has had the good fortune of having his name handed down to the moſt diſtant poſterity, to the prejudice of his predeceſſor. What then will be ſaid, if it ſhall be proved, that neither of thoſe celebrated navigators were the firſt diſcoverers of this immenſe country, and that this honor belongs to a man ſcarcely known in the republic of letters. This, however, is what I ſhall attempt in the following paper; and if the obſcurity of cotemporary writers and the diſtance of time, do not afford arguments ſufficient for an abſolute demonſtration, there will however be enough to call in queſtion the pretenſions of Chriſtopher Columbus.

I ſhall not here enter into an examination of the reveries of ſome hiſtorians, on the voyages of the Carthaginians, the Atlantis of Plato, the bold expedition of Madoc prince of Wales and ſon of Owen Guinnedd, of which Hackluyt has preſerved ſome account, nor on the voyages of Bacchus, or the land Ophir of Solomon. Conjectures of this kind, whether true or falſe, cannot leſſen the glory of Columbus, were there not proof that he received, juſt before

MEMOIRE SUR LA DECOUVERTE DE L'AMERIQUE. MARS 1786.

ON a regardé juſqu'ici comme une injuſtice qu'on n'ait point donnè le nom de Chriſtophe Colomb à la belle partie du monde qu'il a decouverte, et qu'Americ Veſpuce, qui n'a fait que marcher ſur ſes traces, ait eu le bonheur de faire paſſer ſon ſouvenir á la poſterité la plus éloignée au préjudice de ſon prédéceſſeur. Que diroit 'on s'il étoit prouvé qu'aucun de ces grands navigateurs n'a le merite de la premiere decouverte de ce pays immenſe, et que l'honneur en eſt du à un homme preſqu'inconnu dans la republique des lettres? C'eſt ce que je me propoſe de faire dans ce mémoire, et ſi l'obſcurité des écrivains contemporains et l'éloignement des époques ne me permettent pas de pouſſermes argumens juſquâ l'évidence, ils ſuffiront au moins pour établir des doutes fondés ſur la prétendue decouverte de Chriſtophe Colomb.

Nous n'examinerons point ici les rêves de quelques hiſtoriens ſur la navigation des Carthaginois ſur l'Atlantide de Platon, ſur les expeditions hardies de Madox, prince de Galles, et fils d'Owen Guynet, dont Hakluit nous a conſervé les détails, ni ſur les voyages de Bachus ni ſur l'Ophir de Salomon; ces conjectures vraies ou fauſſes ne ſauroient diminuer la glorie de Chriſtophe

before his expedition, the charts and journal of a learned aſtronomer who had been in America.

Garcilaſſo de la Vega, born at Cuſco in Peru, has given us an hiſtory of his country, in which, to take from Columbus the merit of the diſcovery of America, and to give the honor of it to the Spaniards, he aſſures us, that this navigator had been informed of the exiſtence of another continent by Alonzo Sanchez de Huelva, who in his voyage to the Canaries had been driven by a gale of wind to the Antilles; but that his chief information was procured from a celebrated geographer of the name of Martin Behenira. Garcilaſſo ſays nothing more of this Behenira; and ſince we know of no Spaniſh geographer of this name, Garcilaſſo has been ſuſpected of making a ſacrifice of truth to the deſire of wreſting from a Genoeſe the glory of diſcovering the new world.

On looking over, with attention, a liſt of all the learned men of the fifteenth century, I find the name of Martin Behem, a famous geographer and navigator. The chriſtian name is the ſame with that mentioned by Garcilaſſo, and I find that the ſyllables *ira*, added to his name, are owing to a particular circumſtance; namely, the honor conferred on him by John II. king of Portugal. It is then poſſible, that this Martin Behem is the ſame perſon as Martin Behenira mentioned by Garcilaſſo; but this vague conjecture will receive the ſtamp of truth by the following detail. The

Chriſtophe Colomb, s'il n'étoit prouvé, que, peu de tems avant ſon expédition, un aſtronome ſavant avoit été en Amérique, et lui avoit communiqué ſes cartes et ſes journaux.

Garcilaſſo de la Vega, né à Cuſco en Perou, nous a donné une hiſtoire de ſa patrie dans laquelle, pour oter a Colomb le merite de la decouverte de l'Amerique, et pour en faire honneur aux Eſpagnols, il aſſure que ce navigateur avoit été inſtruit de l'exiſtence d'un autre continent par Alonzo Sanchez de Huelva, qui faiſant route pour les Canaries avoit été pouſſé aux Antilles par un coup de vent; mais *qu'il avoit ſur tout tiré grand parti des informations d'un celebre geographe nommé Martin Behenira.* Garcilaſſo ne nous dit rien de plus ſur ce *Behenira*, et comme on ne connoit point de géographe Eſpagnol de ce nom, on a ſoupçonné de la Vega dávoir ſacrifié la verité au deſir de ne pas laiſſer à un Génois la gloire d'avoir decouvert le nouveau monde.

En parcourant avec attention la liſte de tous les ſavans du XV ſiecle, je trouve le nom de *Martin Behem*, grand géographe et navigateur, je trouve que le nom de batême eſt conforme à celui qui eſt cité par Garcilaſſo, que les ſyllabes *ira* ajoutées à ſon nom doivent être dues à une circonſtance particuliere, et cette circonſtance je la trouve dans la confiance dont il a été honnoré par Jean II roi de Portugal. Il eſt donc poſſible que ce *Martin Behem* ſoit le même homme que ce *Martin Behinera* mentionné par Garcilaſſo; mais cette conjecture vague aura tous les caracteres de l'évidence par les détails ſuivans.

L'hiſtoire

The literary hiſtory of Germany gives an account of a Martin Behem, Beheim, or Behin, who was born at Nurenburgh, an imperial city of the circle of Franconia, of a noble family, ſome branches of which are yet extant. He was much addicted to the ſtudy of geography, aſtronomy and navigation, from his infancy. At a more mature age he often thought on the poſſibility of the exiſtance of the Antipodes and of a weſtern continent. Filled with this great idea, he paid a viſit in 1459 to Iſabella daughter of John the I. king of Portugal and regent of the duchy of Burgundy and Flanders. Having informed her of his deſigns, he procured a veſſel, in which he made the diſcovery of the iſland of Fayal in 1460. He there eſtabliſhed a colony of Flemings, whoſe deſcendants yet exiſt in the Azores; which were, for ſome time called the Flemiſh iſlands. This circumſtance is proved, not only by the writings of cotemporary authors, but alſo by the manuſcripts preſerved in the records of Nurenburg, from which the following is copied. "Martin Behem tender-
"ed his ſervices to the daughter of John king of Luſita-
"nia, who reigned after the death of Philip of Burgun-
"dy ſirnamed the Good, and from her procured a ſhip,
"by means of which, having ſailed beyond all the then
"known limits of the weſtern ocean, he was the firſt who,
"in the memory of man, diſcovered the iſland of Fayal,
"abound-

L'hiſtoire litteraire de l'Allemagne nous apprend que *Martin Behem*, *Beheim* ou *Behin* eſt né à Nurenberg, ville imperiale du cercle de Franconie, d'une famille noble dont quelques branches exiſtent encore aujourdhui. Dès ſa plus tendre jeuneſſe il ſe livra à l'étude de la geographie, de l'aſtronomie et de la navigation. Parvenu à un age mur il reflechit beaucoup ſur la poſſibilité de l'exiſtence des Antipodes et d'un continent occidental. Rempli de cette grande idée, i'l fut trouver en 1459 Iſabelle fille de Jean I. roi de Portugal et regente du duché de Bourgogne et de Flandre. Après lui avoir fait part de ſes projets, il en obtint un vaiſſeau avec lequel il fit, en 1460, la decouverte de l'iſle de Fayal. Il y établit une colonie Flamande, dont les deſcendans exiſtent encore aujourdhui aux Açores, qu'on a appellées pendant quelque tems les iſles Flamandes. Cette circonſtance eſt prouvée, non ſeulement par les auteurs contemporains, mais par des manuſcrits conſervés dans les archives de Nurenberg, dont voici la copie: "Martinus "Behemus, Joannis Luſitaniæ regis filiæ, quæ poſt obitum Philippi Burgundi cognomento "boni, rerum dominabatur, operam ſuam addixit, et ab ea navim impetravit, qua occidentalis "oceani hactenus cognitos terminos et fines prætervectus *primus* poſt hominum memoriam "*Fayalem* inſulam, *Fago* arbore, quam Luſitani *Faye* vocant, ac unde appellatio ei hæſit abun-
"dantem

" abounding with beachtrees, which the people of Luſita-
" nia call Faye; whence it derived its name. After this
" he diſcovered the neighbouring iſlands, called by one
" general name *the Azores*, from the multitude of hawks
" which build their neſts there (for the Luſitanians uſe
" this term for hawks, and the French too uſe the word
" *Eſſos* or *Eſores* in their purſuit of this game) and left
" colonies of the Flemiſh on them; when they began to
" be called Flemiſh iſlands, &c." Although this record is contrary to the generally received opinion, that the Azores were diſcovered by Gonſalva Velho, a Portugueſe, yet its authenticity cannot be doubted; it is confirmed by ſeveral cotemporary writers, and eſpecially by Wagenceil, one of the moſt learned men of the laſt century; who after having travelled into Africa, and throughout all Europe, was made doctor of laws at Orleans and choſen fellow of the academy of Turin and Padua, although he was a German by birth. The particulars are to be found in his univerſal hiſtory and geography. I have moreover received, from the records of Nurenberg, a note written in German on parchment, which contains the following facts. " Martin Behem, eſquire, ſon of Mr. Martin Behem of
" Schroperin, lived in the reign of John II. king of Portu-
" gal, in an iſland which he diſcovered, and called the iſland
" of Fayal, one of the Azores, lying in the weſtern ocean."

After having obtained from the regent Iſabella a grant of Fayal, and reſided there about twenty years, during which

" dantem reperit, nec minus poſtea finitimas inſulas, uno nomine ab accipitrum ibi nidifican-
" tium multitudine *Açores* dictas (Luſitani enim hoc vocabulo *accipitres* efferunt, et Galli quo-
" que in aucupiis verbum *Eſſos* et *Eſſores* adhibent) detexit, ac flandrorum colonias in iis re-
" liquit, unde et inſulæ illæ Flandriæ vocari cæperunt, &c." Quoique ce monument ſoit contraire à l'opinion généralement reçue que les Açores ont été découvertes par un Portugais nommé Gonzalve Velho, on ne ſauroit douter de ſon authenticité; il ſe trouve confirmé par pluſieurs auteurs contemporains, et ſurtout par Wagenſeil, un des plus grands ſavans du dernier ſiecle, qui après avoir voyagé par l'Afrique et par toute l'Europe, a été fait docteur en droit à Orleans, et academicien à Turin et à Padoue, quoiqu'il fut né Allemand. On en trouve des détails dans ſon hiſtoire univerſelle et dans ſa geographie. On m'a d'ailleurs communiqué dans les archives de Nurenberg une note en Allemand écrite ſur parchemin, contenant les faits ſuivans: " M. Martin Beham, ecuyer, fils de M. Martin Beham de Scopperin, a vecu ſous le
" regne de Jean II. roi de Portugal, dans une iſle qu'il a trouvée lui même, et qu'il a appellée
" iſle de Fayal; elle eſt ſituée aux Açores dans l'océan occidental."

Aprés avoir obtenu de la regente Iſabelle la proprieté de Fayal, et aprés y avoir employé environ

which time he was buſied in making freſh diſcoveries in geography, by ſmall excurſions, which need not be mentioned, Behem applied in 1484 (which was eight years before Columbus's expedition) to John II. king of Portugal, to procure the means of undertaking a great expedition towards the ſouth-weſt. This prince gave him ſome ſhips, with which he diſcovered that part of America, which is now called Brazil; and he even ſailed to the ſtreights of Magellan, or to the country of ſome ſavage tribes, whom he called Patagonians, from the extremities of their bodies being covered with a ſkin more like a bear's paws than human hands and feet. This fact is proved by authentic records, preſerved in the archives of Nurenberg. One of which in particular deſerves attention "Martin "Behem, traverſing the Atlantic ocean for ſeveral years, "examined the American iſlands, and diſcovered the "ſtrait which bears the name of Magellan, before either "Chriſtopher Columbus or Magellan ſailed thoſe ſeas; "and even mathematically delineated on a geographical "chart for the king of Luſitania, the ſituation of the "coaſt, around every part of that famous and renowned "ſtrait." This aſſertion is ſupported by Behem's own letters, written in German and preſerved in the archives of Nurenberg, in a book which contains the birth and illuſtrious actions of the nobility of that city. Theſe letters

viron 20 années à faire des recherches ulterieures ſur la géographie, dans de petites excurſions que ne meritent par d'être rapportées ici; Behem s'adreſſa en 1484, c'eſt à dire huit années avant l'expedition de Chriſtophe Colomb, à Jean II. roi de Portugal, pour obtenir de lui les moyens d'entreprendre une grande expedition vers le ſud-oueſt. Ce prince lui confia quelques vaiſſeaux avec leſquels il decouvrit la partie de l'Amerique connue ſous le nom de Breſil, et il étendit meme ſa navigation juſqu'au détroit de Magellan, ou à la terre de quelques hordes ſauvages qu'il appella Patagons, puiſque les extremités de leurs corps couvertes de peaux reſſembloient plutôt à des pattes d'ours qu'à des pieds et à des mains. Ce fait eſt prouvé par des documens authentiques dépoſés dans les archives de Nurenberg. Il y en a un ſur tout qui merite notre attention: "Martinus Behemus per oceanum Atlanticum huc illuc annos pluſculos "oberrans, ante Chriſtophorum Columbum Americæ inſulas, ante Fernandum Magellanum, "fretum quod ab eo cognomentum habet perveſtigavit, unde et in *tabula geographica longe prius* "*quam Magellanes* de expeditione ſua cogitaſſet, omnem circa inſigne clariſſimumque fretum "illud oræ habitudinem Luſitaniæ regi radio delineavit." Cette aſſertion ſe trouve appuyée par des lettres de Behem ecrites en Allemand, et conſervées dans les archives de Nurenberg, dans un volume contenant l'origine et les actions éclatantes des patriciens de cette ville. Ces lettres

ters are dated in 1486; that is, six years before the expedition of Columbus. This wonderful discovery has not escaped the notice of cotemporary writers. The following passage is extracted from the chronicle of Hartman Schedl: "In the year 1485, John the second, king of "Portugal, a man of a magnanimous spirit, furnished "some gallies with provisions, and sent them to the south-"ward beyond the straits of Gibraltar. He gave the com-"mand of this squadron to James Canus, a Portuguese, "and Martin Behem a German of Nurenberg in Upper "Germany, descended of the family of Bonna, a man "very well acquainted with the situation of the globe, "blessed with a constitution able to bear the fatigues of "the sea, and who by actual experiments and long sail-"ing, had made himself perfectly master with regard to "the longitudes and latitudes of Ptolemy, in the west. "These two, by the bounty of Heaven, coasting along the "southern ocean, and having crossed the æquator, got "into the other hemisphere, where facing to the east-"ward, their shadows projected towards the south and "right-hand. Thus, by their industry, they may be said "to have opened to us another world hitherto unknown, "and for many years attempted by none but the Genoese, "and by them in vain. Having finished this cruise in the "space of twenty-six months, they returned to Portugal, "with the loss of many of their seamen, by the violence "of the climate." This

lettres sont datées de 1486, c'est à dire six années avant l'expédition de Christophe Colomb. Quelques auteurs contemporains n'ont pas manqué de faire mention d'une decouverte aussi étonnante. Je trouve entre autres dans la chronique de Hartman Schedl le passage suivant: "Anno Domini 1485, Johannes secundus Portugalliæ rex, altissimi vir cordis, certas galeas "omnibus ad victum necessariis instruxit, easque ultra columnas Herculis ad meridiem misit. "Præfecit autem his patronos duos Jacobum Canum Portugallensem et Martinum Behemum, "hominem Germanum ex Nurimberga superioris Germaniæ de Bonna familia natum, hominem "inquam in cognoscendo situ terræ peritissimum, marisque patientissimum, qui Ptolomæi lon-"gitudines et latitudines in occidente ad unguem experimento longævaque navigatione novit. "Hi duo, bono Deorum auspicio mare meridionale sulcantes a littore non longe evagantes "superato circulo æquinoctiali in alterum orbem excepti sunt, ubi ipsis stantibus orientem "versus umbra ad meridiem et dexteram projiciebatur. Aperuere igitur *sua industria alium* "*orbem hactenus nobis incognitum*, et multis annis a nullis quam a *Januensibus licet frustra* tentatum; "peracta autem hujusmodi navigatione vicissimo sexto mense reversi sunt in Portugalliam, "pluribus ob æris impatientiam mortuis."

Cequi

This paſſage becomes more intereſting, from being quoted in a book on the ſtate of Europe during the reign of the emperor Frederick III. by the learned hiſtorian Æneas Sylvius, afterwards pope Pius II. This hiſtorian died before the diſcoveries of Behem were made, but the publiſhers of his works, thought the paſſage in Hartman Schedl ſo important, that they inſerted it in the hiſtory. We alſo find the following particulars, in the remarks made by Petrus Matæus, on the canon law, two years before the expedition of Columbus: "The firſt chriſtian "voyages to the newly diſcovered iſlands became fre- "quent, under the reign of Henry ſon of John king of "Luſitania. After his death, Alphonſus the fifth proſecut- "ed the deſign, and John who ſucceeded him followed "the plan of Alphonſus by the aſſiſtance of Martin Bœhm, "a very experienced navigator, ſo that, in a ſhort time, "the name of Luſitania become famous over the whole "world." Cellarius, one of the moſt learned men of his age, ſays expreſsly: "Bœhm did not think it enough to "ſurvey the iſland of Fayal, which he firſt diſcovered, or "the other adjacent iſlands which the Luſitanians call "Azores, *and we after the example of Bœhm's companions, "call Flemiſh iſlands;* but advanced ſtill farther and far- "ther ſouth, until he arrived at the remoteſt ſtrait, be- "yond which, Ferdinand Magellan, following his tract, "afterwards ſailed and called it after his own name."

Ce qui rend ce paſſage plus intereſſant encore, c'eſt qu'il eſt cité dans l'ouvrage du ſavant hiſtorien *Æneas Sylvius*, depuis pape ſous le nom de Pie II. ſur l'état de l'Europe du tems de l'empereur Frederic III. Cet hiſtorien eſt mort avant les decouvertes de Behem, mais les copiſtes de l'ouvrage d'Æneas Sylvius ont trouvé le paſſage de Hartman Schedl ſi intéreſſant qu'ils l'ont inſeré dans le corps de cette hiſtoire. Nous trouvons d'ailleurs dans les notes que Petrus Matæi a faites ſur le droit canon, deux ans avant l'expedition de Colomb, les détails ſuivans: "Primæ navigationes chriſtianæ ad novas inſulas eluceſcere caperunt ſub Henrico "Johannis Luſitaniæ regis filio, &c. illo mortuo Alphonſus V. cæpta proſecutus eſt; Al- "phonſum Johannes imitatus opera *Martini Bohemi*, hominis in curſu navium peritiſſimi, ut "brevi tempore nihil celebrius per totum orbem audiretur ipſo Luſitaniæ nomine." Cellarius, un des plus grands ſavans de ſon ſiecle, dit expreſſement: "Behaimius non modo "Fagalem inſulam, quam primus invenit, aut alias circumjectas quas Azores Luſitani, nos "*Flandrias a Behaimi comitibus*, nominant, perluſtrandas ſibi cenſuit, verum etiam in auſtrum "magis et magis progreſſus uſque ad ultimum fretum, quod Ferdinandus Magellanus hujus "ductum ſecutus, pertranſiit et de ſuo id nomine appellavit."

Toutes

All theſe quotations, which cannot be thought tedious, ſince they ſerve to prove a fact almoſt unknown, ſeem to demonſtrate, that the firſt diſcovery of America is due to the Portugueſe, and not to the Spaniards; and that the chief merit belongs to a German aſtronomer. The expedition of Ferdinand Magellan, which did not take place before the year 1519, aroſe from the following fortunate circumſtance. This perſon, being in the apartment of the king of Portugal, ſaw there a chart of the coaſt of America, drawn by Behem, and at once conceived the bold project of following the ſteps of this great navigator. Jerome Benzon, who publiſhed a deſcription of America in 1550, ſpeaks of this chart, a copy of which, ſent by Behem himſelf, is preſerved in the archives of Nurenberg. The celebrated aſtronomer Riccioli, though an Italian, yet does not ſeem willing to give his countryman the honor of this important diſcovery. In his geography reformed, book III. page 90, he ſays: "Chriſtopher Columbus never thought "of an expedition to the Weſt Indies, until ſome time "before, while in the iſland of Madeira, where amuſing "himſelf in forming and delineating geographical charts, "he obtained information from Martin Bœhm, or as the "Spaniards ſay, from Alphonſus Sanchez de Huelva, a "pilot, who by meer chance had fallen in with the iſland "afterwards called Dominica." And in another place, "let Bœhm and Columbus have each their praiſe, they "were

Toutes ces citations qui ne ſauroient être trop longues, parcequ'elles ſervent à prouver un fait preſqu'inconnu, paroiſſent demontrer que la premiere decouverte de l'Amerique eſt due aux Portugais, et non aux Eſpagnols; et que c'eſt un aſtronome Allemand qui étoit à leur tête. L'expedition de Ferdinand Magellan, qui n'a eu lieu qu'en 1519, eſt due à un heureux hazard. Ce navigateur ſe trouvant dans l'appartement du roi de Portugal, y vit une carte des cotes de l'Amerique tracée par Behem, et conçut dès lors le projet hardi de ſuivre la route de ce grand navigateur. Jerome Benzon, qui a donnée en 1550, une deſcription de l'Amerique, fait mention de cette carte, dont on a conſervé une copie dans les archives de Nurenberg, où Behem l'avoit envoyée lui-même; le celebre aſtronome Riccioli, qui étoit lui-même Italien, ne paroit pas cependant vouloir attribuer à ſon compatriote cette importante decouverte; il dit dans ſa geographie reformée, livre 3. p. 90. "Chriſtophorus Columbus, cum "prius in Madera inſula, ubi conficiendis et delineandis chartis geographicis vacabat, *indicio ha-*"*bito a Martino Bohemo*, aut *ut Hiſpani dicitant* ab Alphonſo Sanchez de Huelva, nauclero "qui forte inciderat in inſulam, poſtea *Dominicam* dictam, cogitavit de navigatione in Indiam "occidentalem." Et dans un autre endroit, "Sit ſua laus Bohemo, ſit ſua laus Columbo, "ambo

" were both excellent navigators; but Columbus would " never have thought of his expedition to America, had " not Bœhm gone there before him. His name is not ſo " much celebrated as that of Columbus, Americus or Ma- " gellan, although he is ſuperior to them all."

But the moſt poſitive proof of the great ſervices rendered to the crown of Portugal by Behem, is the recompence beſtowed on him by king John, who in 1485 knighted him in the moſt ſolemn manner, in the preſence of all his court. I have before me a German paper extracted from the archives of Nurenberg to the following purpoſe. " In the year 1485, on the 18th of February, in " Portugal, in the city of Allaſavas, and in the church of " St. Salvador, after the maſs, Martin Behem of Nurenberg, " was made a knight by the hands of the moſt puiſſant " lord, John the ſecond king of Portugal, Algarve, Afri- " ca and Guinea; and his chief ſquire was the king him- " ſelf, who put the ſword in his belt; and the duke of Begia " was his ſecond ſquire, who put on his right ſpur; and " his third ſquire was count Chriſtopher de Mela, the " king's couſin, who put on his left ſpur; and his fourth " ſquire was count Martini Marbarinis who put on his " iron helmet; and the king himſelf gave him the blow " on the ſhoulder, which was done in the preſence of all " the princes, lords and knights of the kingdom: and he " eſpouſed

" ambo fuerunt magni navarchi, ſed nunquam de ſua in Americam expeditione cogitaſſet " Columbus niſi Bohemum habuiſſet prædeceſſorem. Hujus nomen non tantopere celebra- " tur quanto Columbi, Americi et Magellani, quamvis his tribus ſit præferendus."

Mais ce qui prouve plus que toute autre choſe les grands ſervices rendus par Behem à la couronne de Portugal, c'eſt la reconnoiſſance du roi Jean, qui en 1485, le fit lui-même chevalier, de la maniere la plus ſolemnelle, et en preſence de toute ſa cour. J'ai ſous mes yeux un document Allemand, tiré des archives de Nurenberg, de la tenur ſuivante: " En 1485, " le 18 Fevrier, en Portugal dans la ville d'Allafavas, et dans l'égliſe de Santo Salvador, " après la meſſe, a été fait chevalier Martino Behem, de Nurenberg, par la main du très " puiſſant ſeigneur roi Jean ſecond de Portugal, roi d'Algarve, roi d'Afrique et roi de " Guinée; et ſon premier ecuyer etoit le roi lui-même, qui mit ſon épée à ſon ceinturon; et " le duc de Begia étoit ſon ſecond ecuyer qui lui mit ſon épéron droit; et ſon troiſieme " ecuyer étoit le comte Chriſtophe de Mela couſin du roi, qui lui mit ſon épéron gauche; et " ſon quatrieme ecuyer étoit le comte Martini Marbarinis, qui lui mit ſon caſque de fer; et " le roi lui donna lui-même un coup ſur l'epaule; ce qui ſe paſſa en preſence de tous les princes " et de tous les ſeigneurs et chevaliers du royaume; et il a eſpouſé la fille d'un grand ſeigneur " en

" eſpouſed the daughter of a great lord, in conſideration " of the important ſervices he had performed, and he was " made governor of the iſland of Fayal." Theſe marks of diſtinction conferred on a ſtranger, could not be meant as a recompence for the diſcovery of the Azores, which was made 20 years before; but as a reward for the diſcovery of Congo, from whence the chevalier Behem had brought gold and different kinds of precious wares. This diſcovery made much greater impreſſion than that of a weſtern world, made at the ſame time, but it neither increaſed the wealth of the royal treaſury, nor ſatisfied the avarice of the merchants.

In 1492 the chevalier Behem, crowned with honors and riches, undertook a journey to Nurenberg, to viſit his native country and his family. He there made a terreſtrial globe, which is looked on as a maſter-piece for that time, and which is ſtill preſerved in the library of that city. The tract of his diſcoveries may there be ſeen under the name of weſtern lands, and from their ſituation it cannot be doubted, that they are the preſent coaſts of Brazil and the environs of the ſtraits of Magellan. This globe was made in the ſame year that Columbus ſat out on his expedition, from whence it is not poſſible that Behem could have profited by the works of this navigator, who beſides, went a much more northerly courſe.

After having performed ſeveral other intereſting voyages, the chevelier Behem died at Liſbon in July 1506, regretted

" en conſidération des ſervices qu'il a rendus, et il a été fait gouverneur de l'iſle de Fayal." Cette grande diſtinction accordée à un étranger, ne pouvoit être la recompenſe de la decouverte des Açores, qui avoit eu lieu plus de 20 ans auparavant; mais elle étoit le pris de la decouverte du Congo, d'ou le chevalier Behem avoit apporté de l'or et pluſieurs marchandiſes précieuſes. Cette decouverte fit beaucoup plus d'impreſſion que celle d'une terre occidentale faite dans le meme tems, mais qui n'offroit aucun benefice au tréſor royal ni à la cupidité des marchands.

En 1492, le chevalier Behem, comblé d'honneur et de richeſſes, entreprit un voyage à Nurenberg, pour revoir ſa patrie et ſa famille. Il y compoſa un globe terreſtre, qui eſt regardé comme un chef d'œuvre de ſon tems, et qui eſt encore conſervé dans la bibliotheque de cette ville. On y voit la trace de ſes decouvertes ſous le nom de terres occidentales, et par leur ſituation on ne peut diſconvenir qu'elles ne ſoient les cotes actuelles du Breſil et les environs du détroit de Magellan. Ce globe eſt fait dans la même année où Colomb a commençé ſon expedition; il eſt donc impoſſible que Behem ait profité du travail de ce navigateur, qui d'ailleurs a dirigé ſa courſe beaucoup plus au nord.

Après avoir achevé pluſieurs autres voyages intéreſſans, le chevalier Behem mourut à Liſbonne

regretted by every body, but leaving behind him no other work than the globe which we have juſt been ſpeaking of. It is made from the writings of Ptolemy, Pliny, Strabo, and eſpecially from the account of Mark Paul the Venetian, a celebrated traveller of the XIIIth century, and of John Mandeville, an Engliſhman, who, about the middle of the XIVth century, publiſhed an account of a journey of 33 years in Africa and Aſia. He has alſo added the important diſcoveries made by himſelf on the coaſts of Africa and America.

From theſe circumſtantial accounts, little known to modern writers, we muſt conclude that *Martin Behenira*, of whom Garcilaſſo makes mention, is the ſame chevalier Behem, upon being the place of whoſe birth Nurenberg prides itſelf ſo much. It is probable, that as ſoon as he was knighted in Portugal, he thought it neceſſary to give a Portugueſe termination to his name, to make it more ſonorous and more conformable to the idiom of the country. Garcilaſſo, deceived by this reſemblance of ſound, has made him a Spaniard, in order to deprive Chriſtopher Columbus of the honor of having procured to his country ſo great an advantage. And what ought to confirm us in this opinion is, that we neither find in Mariana nor any other Spaniſh hiſtorian, the name of this Martin Behemira, who was certainly a man of too much importance not to have had a diſtinguiſhed place in hiſtory. Beſides, the Spaniſh pride

bonne en Juillet 1506, generalement regretté, mais ne laiſſant à la poſterité d'autre ouvrage que le globe dont nous venons de parler. Il eſt fait d'après les ecrits de Ptolomée, de Pline, de Strabon, et ſurtout d'après les relations du Venitien Marc Paul, voyageur celebre du XIII ſiecle, et de Jean Mandeville, Anglois, qui au milieu du XIV ſiecle a publié les détails d'un voyage de 33 années en Afrique et en Aſie; il y a ajouté, les grandes decouvertes qu'il a faites lui-même ſus les cotes d'Afrique et d'Amérique.

D'après ces détails peu connus des écrivains modernes, nous devons conclure que *Martin Behemira*, dont Garcilaſſo fait mention, eſt ce même chevalier Behem que la ville de Nurenberg ſe glorifie d'avoir vu naitre dans ſes murailles. Il eſt vraiſemblable qu'au moment où il fut creé chevalier en Portugal, il a cru devoir donner une terminaiſon Portugaiſe à ſon nom, pour le rendre plus ſonore et plus conforme à l'idiome du pays. Garcilaſſo trompé par cette reſemblance de ſon, en a fait un Eſpagnol pour enlever à Chriſtophe Colomb la gloire d'avoir procuré à ſa métropole un auſſi grande acquiſition. Ce qui doit nous confirmer dans cette opinion, c'eſt que nous ne trouvons ni dans Mariana ni dans aucun autre hiſtorien Eſpagnol le nom de ce *Martin Behemira* qui auroit du être un homme trop important pour ne pas occuper

pride would have been flattered in giving to a native thoſe laurels with which it crowned Chriſtopher Columbus.

It is then very unlikely, that this navigator was treated as an enthuſiaſt, when he offered to the court of Portugal to make diſcoveries in the weſt. The ſearch after unknown countries was at that time the reigning paſſion of this court; and even if the chevalier Behem had not offered the intereſting ideas which he had procured, the novelty of the project had undoubtedly engaged king John to give into the views of Columbus; but it appears that this prince declined it, becauſe all his thoughts were turned at that time to the coaſt of Africa, and the new paſſage to the Indies, from whence he promiſed himſelf great riches; whilſt the ſouthern coaſt of Brazil and the territories of the Patagonians, ſeen by Behem, offered to him only barren lands, inhabited by unconquerable ſavages. The refuſal of John II. very far from weakening the teſtimony of Behem's diſcoveries, is then rather a proof of the knowledge, which this politic prince had already procured, of the exiſtence of a new continent; and it was only in 1501, that is to ſay three years after the voyage of Vaſco de Gama to the Indies, that Emanuel thought proper to take advantage of the diſcoveries of Behem, by ſending Albarez Cabral to Brazil; a meaſure which was perhaps rather owing to the jealouſy which has always exiſted between Portugal and Spain,

cuper une place diſtinguée dans l'hiſtoire. La fierté Eſpagnole auroit d'ailleurs été flattée d'accorder à un national les lauriers dont elle a couronné Chriſtophe Colomb.

Il eſt donc peu vraiſemblable que ce navigateur ait été traité comme un extravagant quand il offrit à la cour de Portugal de faire des découvertes dans l'oueſt. La recherche des pays inconnus étoit alors la paſſion dominante de cette cour, et quand même le chevalier Behem n'auroit pas donné les notions importantes qu'il s'étoit procurées, la nouveauté du projet eut indubitablement engagé le roi Jean à ſe prêter aux vues de Colomb; mais il paroit que ce prince s'y eſt refuſé, puiſque toutes ſes vues portoient alors ſur la cote d'Afrique et le nouveau paſſage dans l'Inde, d'où il ſe promettoit de tirer de grandes richeſſes, tandiſque les cotes meridionales du Breſil et la terre des Patagons, vues par Behem, ne lui offroient que des terres ſteriles, habitées par des ſauvages indomptables. Le refus de Jean II. bien loin d'affoiblir l'evidence des decouvertes de Behem, eſt donc plutot une preuve des connoiſſances que ce prince habile s'étoit déjà procurées ſur l'exiſtence d'un nouveau continent; et ce n'eſt qu'en 1501, c'eſt à dire trois ans aprés l'expedition de Vaſco de Gama dans l'Inde, qu'Emanuel jugea à propos de tirer parti des decouvertes de Behem, en envoyant au Breſil Albarez Cabral; meſure qui étoit peut être plutôt une ſuite de cette jalouſie qui a toujours exiſté entre

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Spain, than to a desire of making advantageous establishments, for which the Indies were much more proper than this part of America.

If any doubts yet remain respecting the important discovery made by the chevalier Behem, it is particularly the authority of Dr. Robertson, which attacks the testimony of the different authors we have transcribed. This learned writer treats the history of Behem as a fiction of some German authors, who had an inclination to attribute to one of their countrymen, a discovery, which has produced so great a revolution in the commerce of Europe. But he acknowledges nevertheless, with Herrera, that Behem had settled at the island of Fayal, that he was the intimate friend of Christopher Columbus, and that Magellan had a globe made by Behem, by the help of which he undertook his voyage to the South sea; a circumstance which proves much in favor of our hypothesis. He relates also, that in 1492, this astronomer paid a visit to his family at Nurenberg, and left there a map drawn by himself, which Dr. Forster procured him a copy of, and which, in his opinion, partakes of the imperfection of the cosmographical knowledge of the fifteenth century; that he found in it, indeed, under the name of the island of St. Brandon, land which appears to be the present coast of Guiana, and lies in the latitude of cape Verd, but that there is reason to believe, that this fabulous island, which

le Portugal et l'Espagne, que du desir de faire des établissemens avantageux aux quels l'Inde étoit beaucoup plus propre que cette partie de l'Amerique.

S'il nous est permis de douter encore de l'importante decouverte faite par le chevalier Behem, c'est surtout l'autorité du Dr. Robertson, qui doit porter atteinte aux témoignages des differens auteurs que nous avons transcrits. Ce savant écrivain traite l'histoire de Behem comme une fiction de quelques auteurs Allemans, qui desiroient d'attribuer à un de leurs compatriotes une decouverte qui a produit une si grande revolution dans le commerce de l'Europe. Mais il avoue cependant, d'après Herera, que Behem étoit établi à l'isle de Fayal, qu'il étoit *l'ami intime de Christophe Colomb*, et que Magellan avoit eu un globe composé par Behem, d'après lequel il avoit entrepris son expedition dans la mer du Sud; circonstance qui prouve beaucoup en faveur de notre hypothese. Il rapporte de plus qu'en 1492, cet astronome a été voir sa famille à Nurenberg, et qu'il y a laissé une carte dessinée par lui-même; que le Dr. Forster lui a procuré une copie de cette carte, qui suivant lui se ressent de l'imperfection des connoissances cosmographiques du XV siecle; qu'il y a trouvé à la verité sous le nom de l'isle de St. Brandon, une terre qui paroit être la cote actuelle de la Guyane, et qui est placée dans la latitude

is found in many ancient maps, merits no more attention than the childiſh legend of St. Brandon himſelf. Although Dr. Robertſon does not appear diſpoſed to grant to Behem the honor of having diſcovered the new continent, we find the means of refuting him in his own hiſtory. He allows that Behem was very intimate with Chriſtopher Columbus, that he was the greateſt geographer of his time, and ſcholar of the celebrated John Muller or Regiomontanus; that he had diſcovered, in 1483, the kingdom of Congo upon the coaſt of Africa; that he made a globe, which Magellan made uſe of; that he drew a map at Nurenberg containing the particulars of his diſcoveries, and that he placed in this chart land which is found to be in the latitude of Guiana. Dr. Robertſon aſſerts, without any proof, that this land was but a fabulous iſland; we may ſuppoſe, upon the ſame foundation, that the chevalier Behem, engaged in an expedition to the kingdom of Congo, was driven by the winds to Fernambouc, and from thence, by the currents, very common in thoſe latitudes, towards the coaſt of Guiana; and that he took for an iſland the firſt land which he diſcovered. The courſe which Chriſtopher Columbus afterwards ſteered, makes this ſuppoſition ſtill more probable; for if he knew only of the coaſt of Brazil, which they believe to have been diſcovered by Behem, he would have laid his courſe rather to the ſouth-weſt. The expedition

titude du cap Verd; mais qu'il y a lieu de croire, que cette iſle fabuleuſe, qui ſe trouve ſur pluſieurs anciennes cartes, ne merite pas plus d'attention que la legende puerile de St. Brandon lui-même. Quoique le Dr. Robertſon ne paroiſſe pas diſpoſé d'accorder à Behem la gloire d'avoir decouvert le nouveau continent, nous trouvons dans ſon hiſtoire même des armes pour le combattre. Il convient que Behém étoit très lié avec Chriſtophe Colomb, qu'il étoit le plus grand geographe de ſon tems, et diſciple du celebre Jean Muller ou Regiomontanus; qu'il a decouvert en 1483, le royaume de Congo, ſur la cote d'Afrique; qu'il a compoſé un globe dont s'eſt ſervi Magellan; qu'il a deſſiné á Nurenberg, une carte contenant des détails ſur ſes decouvertes, et qu'il a marqué ſur cette carte une terre qui ſe trouve dans la latitude de la Guyane actuelle. Le docteur Robertſon admet ſans aucune preuve, que cette terre n'étoit qu'une iſle fabuleuſe; nous pouvons ſuppoſer avec autant de fondement, que le chevalier Behem faiſant ſon expedition dans le royaume de Congo, ait été pouſſé par les vents vers Fernambouc, et de là par des courans très communs dans ces parages vers les cotes de la Guyane, et qu'il ait pris pour une iſle la premiere terre qui s'eſt offerte à ſes yeux. La route qu'a pris dans la ſuite Chriſtophe Colomb rend cette ſuppoſition encore plus vraiſemblable, car s'il n'avoit eu connoiſſance que des cotes du Breſil, que l'on croit avoir été reconnues par Behem, il auroit dirigé ſa navigation plutôt vers le ſud-oueſt. L'expedition au Congo a eu lieu

expedition to Congo took place in 1483; it is then poſſible, that, at his return, Behem propoſed a voyage to the coaſts of Brazil and Patagonia, and that he requeſted the aſſiſtance of his ſovereign, which we have mentioned above. It is certain, that we cannot have too much deference for the opinion of ſo eminent a writer as Robertſon, but this learned man not having it in his power to conſult the German pieces in the original, which we have quoted, we may be allowed to form a different opinion without being too preſumptuous.

But ſhould it be aſked, why we take from Chriſtopher Columbus the reputation which all Europe has to this day allowed him? Why we are ſearching in the archives of an imperial city, for the cauſes of an event which took place in the moſt weſtern extremity of Europe? Why the enemies of Chriſtopher Columbus, who were numerous, did not take advantage of the pretended chevr. Behem, to leſſen his conſequence at the Spaniſh court? Why Portugal, jealous of the diſcovery of the new world, had not proteſted againſt the aſſertions of the Spaniards? Why Behem, who died only in 1506, had not left to poſterity any writing to confirm to himſelf ſo important a diſcovery?

To anſwer all theſe queſtions, I ſhall ſubmit to the impartial reader, the following remarks:

lieu en 1483, il eſt donc poſſible qu'à ſon retour Behem ait projetté une expedition vers les cotes du Breſil et des Patagons, et qu'il ait demandé à ſon ſouverain les ſecours dont nous avous parlé plus haut. Il eſt ſur qu'on ne ſauroit avoir trop de déference pour l'opinion d'un écrivain tel que Robertſon, mais ce ſavant n'ayant pu avoir connoiſſance des pieces Allemandes originales que nous avous citées, nous pouvons avoir un avis different du ſien ſans nous rendre coupable de preſomption.

Mais, dira-ton, pourquoi enlever à Chriſtophe Colomb une gloire que toute l'Europe lui a accordée juſqu'ici? Pourquoi chercher dans les archives d'une ville imperiale les cauſes d'un évenement qui a eu lieu à l'extremité la plus occidentale de l'Europe? Pourquoi les ennemis de Chriſtophe Colomb, qui étoient en grand nombre, n'ont 'ils pas tiré parti des prétendues decouvertes du Chevalier Behem pour diminuer ſon importance à la cour d'Eſpagne? Pourquoi le Portugal, jaloux de la decouverte du nouveau monde, n'at'il pas proteſté contre les aſſertions des Eſpagnols? Pourquoi Behem qui n'eſt mort qu'en 1506, n'at'il pas laiſſé lui même à la poſterité un écrit pour s'attribuer une decouverte auſſi importante?

Pour repondre à toutes ces queſtions je ſoumettrai au lecteur impartial les remarques ſuivantes:

1. Before Columbus, the great merit of a navigator conſiſted rather in conceiving the poſſibility of the exiſtence of a new continent, than in ſearching for lands in a region where he was ſure to find them. If it is then certain that Behem had conceived this bold idea before Columbus, the fame of the latter muſt be conſiderably diminiſhed.

2. The hiſtorical proofs, which we have given above, leaving us no doubt of the fact, we have only to explain the moral cauſes of the ſilence of the Spaniſh and Portugueſe authors, of the enemies of Columbus, and of Behem himſelf.

3. It is well known, that previous to the reign of Charles V. there was little communication between the learned men of different nations. Writers were ſcarce, excepting ſome monks who have related, well or ill, the events which came to their knowledge, in chronicles which are no longer read; or they had but little idea of what paſſed in foreign countries. Gazettes and journals were unknown, and the learned obliged to travel to inform themſelves of the progreſs of their neighbours. Italy was the center of the arts and what are called ſcience at that time. The frequent journies of the German emperors to Rome gave them an opportunity of knowing perſons of merit, and of placing them in the different univerſities of the empire. It is to this circumſtance that we ought

1. Avant Colomb le grand merite d'un navigateur conſiſtoit plutôt à conçevoir la poſſibilité de l'exiſtence d'un nouveau continent, qu'à chercher des terres dans une region où il etoit ſur d'en trouver. S'il eſt donc certain que Behem a eu cette idée hardie avant Colomb, la gloire de ce dernier en eſt ſingulierement diminuée.

2. Toutes les preuves hiſtoriques que nous avous donnés ci deſſus ne nous laiſſant aucun doute ſur le fait, il s'agit ſeulement d'expliquer les cauſes morales du ſilence des auteurs Eſpagnols et Portugais, des énnemis de Colomb, et de Behem lui-même.

3. On ſait qu'avant Charles quint il y avoit très peu de communication entre les ſavans des differentes nations. Les ecrivains étoient fort rares, à l'exception de quelques moines qui ont rapporté bien ou mal les evenemens qui étoient à leur portée dans des chroniques qu'on ne lit plus, ou n'avoit que peu de notions ſur ce qui ſe paſſoit en pays étranger; les gazettes, les journaux étoint inconnus, et les ſavans étoient obligés de voyager pour voir de leurs propres yeux les progrés de leurs voiſins. L'Italie étoit le centre des arts et de ce qu'on appelloit ſcience dans ce tems-là. Les frequens voyages des empereurs d'Allemagne à Rome leur donna la facilité de connoitre des gens de merite, et de les placer dans les differentes univerſités de

ought to attribute the great progreſs which the Germans made, particularly in mathematics, from the fourteenth to the ſixteenth century; during which time they had the beſt geographers, the beſt hiſtorians, and the moſt enlightened politicians. They were particularly attentive to what paſſed in Europe, and the multiplied connections of different princes with foreign powers, aſſiſted them greatly in collecting in their archives the original pieces of the moſt important events of Europe. It is to this ſpirit of criticiſm and enquiry, that we are indebted for the reformation of Luther, and we cannot deny, that particularly in the fifteenth century, there was more hiſtorical and political knowledge in Germany than in all the reſt of Europe, Italy excepted. It is not then aſtoniſhing, that we ſhould find, in the archives of one of the moſt ancient imperial cities, the particulars of an expedition, planned upon the banks of the Tagus by a German, a man of great repute in his own country, and whoſe every action became very intereſting.

4. It was different in Portugal, where the whole nation, except the king, was plunged in the moſt profound ignorance. Every body was either ſhopkeeper, ſailor or ſoldier; and if this nation has made the moſt important diſcoveries, we muſt aſcribe them rather to avarice than to a deſire of knowledge. They were ſatisfied with ſcraping

de l'empire. C'eſt à cette circonſtance que l'on doit attribuer les grands progrés que les Allemans ont faits ſurtout dans les mathematiques, depuis le XIV. juſqu'au XVI. ſiecle; ils avoient les meilleurs géographes, les meilleurs hiſtoriens et les politiques les plus éclairés. Ils étoient attentifs ſur tout ce qui ſe paſſoit en Europe, et les liaiſons multipliées des differens princes avec les puiſſances étrangeres, leur donnoient une grande facilité de raſſembler, dans leurs archives, les pieces originales des evenemens les plus importans de l'Europe. C'eſt a cet eſprit de critique et de recherche qu'eſt due en grande partie la reformation de Luther, et on ne peut ſe diſſimuler, que, ſurtout dans le XV. ſiecle, il n'y ait eu plus de connoiſſances hiſtoriques et politiques en Allemagne que dans tout le reſte de l'Europe, à l'exception de l'Italie. Il n'eſt donc pas étonnant que nous trouvions dans les archives d'une des plus anciennes villes imperiales des détails ſur une expedition, projetté ſur les bords du Tage par un Allemand, par un homme très conſidéré dans ſon pays, et dont par conſequent toutes les demarches devenoient intereſſantes.

4. Il n'en étoit pas de même du Portugal, où toute la nation à l'exception du ſouverain, étoit plongée dans la plus profonde ignorance. Tout le monde y étoit ou marchand, ou matelot, ou ſoldat; et ſi ce peuple a fait les decouvertes les plus importantes, il faut en chercher le motif dans ſa cupidité, et non dans ſon deſir de s'inſtruire. Il ſe contenta d'amaſſer de l'or dans

ing together gold in every quarter of the known world, whilſt the German and the Italian took up the pen, to tranſmit to poſterity the remembrance of their riches and cruelties. The Spaniards were not much more informed before Charles V. introduced at Madrid the learned men of Flanders and Germany. It is then very poſſible, that the chevr. Behem made very intereſting diſcoveries in geography, in 1485, without the public's being acquainted with them. If he had brought back from his expedition, gold or diamonds, the noiſe would have been ſpread in a few weeks; but ſimple geographical knowledge was not of a nature to intereſt men of this turn of mind.

5. The long ſtay which Chriſtopher Columbus made at Madeira, makes his interview with Behem more than probable. It is impoſſible that he ſhould have neglected ſeeing a man ſo intereſting, and who could give him every kind of information, for the execution of the plan which he had formed. The mariners who accompanied the chevr. Behem, might alſo have ſpread reports at Madeira and the Azores, concerning the diſcovery which they had been witneſſes of. What ought to confirm us in this, is, that *Mariana* ſays himſelf (book 26. chap. III.) that a *certain* veſſel going to Africa, was thrown by a gale of wind upon certain unknown lands, and that the ſailors at their return to Madeira had communicated to Chriſtopher Columbus

dans toutes les parties du monde connu, tandiſque l'Allemand et l'Italien tenoient la plume pour tranſmettre à la poſterité le ſouvenir de ſes richeſſes et de ſes cruautés. Les Eſpagnols n'étoient par beaucoup plus inſtruits, avant que Charlesquint eut amené à Madrid des ſavans de Flandres et d'Allemagne. Il eſt donc très poſſible que le chevalier Behem ait fait en 1485, des decouvertes trés intéreſſantes pour la géographie, ſans que le public en ait été inſtruit. S'il eut rapporté de ſon expedition de l'or ou des diamans, le bruit s'en ſeroit repandu en peu de ſemaines, mais de ſimples notions géographiques n'étoient pas de nature à intéreſſer des hommes de cette trempe.

5. Le long ſéjour qu'a fait Chriſtophe Colomb à Madere, rend ſon entrevue avec Behem plus que vraiſemblable. Il eſt impoſſible qu'il ait négligé de voir un homme auſſi intéreſſant, et qui pouvoit lui donner toutes ſortes de conſeils ſur l'execution du plan qu'il avoit formé. Les marins qui ont accompagné le chevalier Behem pouvoient d'ailleurs repandre à Madere, et aux Açores, des bruits concernant les decouvertes dont ils avoient été témoins. Ce qui doit nous confirmer dans cette opinion, c'eſt que *Mariana* dit lui-même (livre 26. chap. 5.) qu'un *certain* batiment allant en Afrique, avoit été jetté par un coup de vent ſur de certaines terres inconnues; et que les matelots après leur retour à Madere, avoient communiqué à Chriſtophe

lumbus the circumſtances of their voyage. All authors agree that this learned man had ſome information reſpecting the weſtern ſhores, but they ſpeak in a very vague manner. The expedition of the chevr. Behem explains this myſtery.

6. This aſtronomer could not be jealous of the diſcoveries of Columbus, becauſe the laſt had been farther north, and that in a time when they did not know the whole extent of the new world, and when geographical knowledge was extremely bounded, it might be believed, that the country diſcovered by Columbus, had no connection with that diſcovered by Behem.

It appears however, certain, that Behem diſcovered this continent before Columbus, and that this queſtion, which is only curious in Europe, becomes intereſting to the American patriot. The Grecians have carefully preſerved the fabulous hiſtory of their firſt founders, and have raiſed altars to them; why are not Behem, Chriſtopher Columbus and Veſpucius, deſerving of ſtatues, in the public ſquares of American cities? Theſe precious monuments would tranſmit to poſterity the gratitude which the names of theſe benefactors of mankind ſhould inſpire. Without knowing it, they have laid the foundation of the happineſs of many millions of inhabitants; and Seſoſtris, Phul, Cyrus, Theſeus and Romulus, the founders of the greateſt

Chriſtophe Colomb les circonſtances de leur navigation. Tous les auteurs s'accordent en général, que ce ſavant avoit eu quelques renſeignemens ſur des terres occidentales, mais ils ne nous en parlent que d'une maniere très vague. L'expedition du chevalier Behem nous explique ce myſtere.

6. Cet aſtronome ne pouvoit être jaloux des decouvertes de Colomb, puiſque celui-ci a été beaucoup plus nord; et que dans un tems où l'on ne connoiſſoit par toute l'étendue du nouveau monde, et où les connoiſſances géographiques étoient extrêmement bornées, ou pouvoit croire que le pays trouvé par Colomb n'avoit aucun rapport avec celui de Behem.

Quoiqu'il en ſoit, il paroit certain que Behem a decouvert ce continent avant Colomb, et que cette queſtion qui n'eſt que de pure curioſité en Europe, devient intéreſſante pour le patriote Américain. Les Grecs ont conſervé ſoigneuſement l'hiſtoire fabuleuſe de leurs premieres fondateurs, ils leur ont élevé des autels; pourquoi Behem, Chriſtophe Colomb et Veſpuce ne meriteroient'-ils pas des ſtatues ſur les places publiques des villes Americaines? Ces monumens precieux tranſmettroient à la poſterité la reconnoiſſance que doivent inſpirer les noms de ces bienfaiteurs du genre humain. Sans le ſavoir ils ont jetté les fondemens du bonheur de pluſieurs millions d'habitans; et Seſoſtris, Phul, Cyrus, Theſée, Romulus, les fondateurs

greateſt empires, will be forgotten, before the ſervices rendered by theſe illuſtrious navigators can be effaced from the memory of man.

fondateurs des plus grands empires, ſeront oubliés, avant que les ſervices rendus par ces navigateurs illuſtres, puiſſent s'effacer de la memoire des hommes.

N° XXXVI.

The antiſeptic Virtues of Vegetable Acid and Marine Salt combined, in various Diſorders accompanied with Putridity; communicated in a Letter to JOHN MORGAN, *M.D. F.R.S. and Profeſſor of the Theory and Practice of Phyſic at Philadelphia, by* WILLIAM WRIGHT, *M. D. of Trelawney in Jamaica.*

HAVING experienced the virtues of vegetable acid and marine ſalt, when combined; I beg leave to lay before you a few obſervations on the uſe of this ſimple medicine in ſeveral diſeaſes. It is my ſincere wiſh, that it may prove as beneficial to mankind in general, as it has been to many of my patients in this part of the country.

Take of lime-juice or lemon-juice three ounces, of marine ſalt as much as the acid will diſſolve; of any ſimple diſtilled cordial water one pint; and of loaf ſugar a ſufficient quantity to ſweeten it. The doſe of this mixture muſt be proportioned to the age, ſex, and violence of the diſeaſe. A wine glaſs-ful may be given to adults every two, four or ſix hours.

By Geoffroy's table it appears, that the foſſil alkali has a greater affinity with the marine, than with the vegetable acid. However, marine ſalt diſſolves readily in the lime-juice, throws up a white ſcum to the ſurface, and on applying the ear near the veſſel where the experiment is made, a ſmall hiſſing may be heard, ſimilar to that when acids

acids and alkalies are mixed. It would ſeem probable that part of the marine ſalt is hereby decompoſed.

That vegetable acids and marine ſalt are antiſeptics, has long been known, but their effects when mixed I apprehend to be but lately diſcovered.

Without farther preface, I ſhall proceed to the particular diſeaſes in which they have been adminiſtered, prepared as above.

Of the DYSENTERY.

The dyſentery is a very frequent diſorder in this and other Weſt-India iſlands; and ſometimes is epidemic, particularly in the rainy ſeaſons, or when proviſions are ſcarce. Amongſt other cauſes of dyſenteries, I have often known the eating of yams not arrived at maturity, as alſo unripe alligator pears, produce a bloody flux.

Dyſenteries commonly begin with frequent looſe ſtools for a day or two, attended with gripings: by degrees, the gripes grow more ſevere, nothing is voided by ſtool but a ſmall quantity of mucus, mixed with blood; a teneſmus comes on and is exceedingly troubleſome.

The appetite fails, the patients are low ſpirited, and ſuffer a great proſtration of ſtrength. The mouth and tongue are much furred and ſlimy, and the taſte is like that of rotten butchers meat. The deſire of drink is ſometimes exceſſive, but for the moſt part very moderate. The pulſe is very low, feeble and undulating; and rarely riſes ſo high, as to indicate the uſe of a lancet. Such was the dyſentery in 1771. It proved fatal to many people, both old and young, though treated according to the moſt approved methods of cure, and the loſs of ſeveral patients of mine, convinced me of the neceſſity of uſing antiſeptics early in this diſeaſe.

A vomit ſeemed neceſſary to clear the ſtomach, and ſome gentle purge, to carry off part of the offending mat-

ter by ſtool. But the action of theſe, however mild, often increaſed the proſtration of ſtrength, and rendered the ſtools ſooner bloody. Nor was opium of any real uſe. A tea made of Simarouba and given to ſome, had a very ſalutary effect, whilſt, if given to others, it would by no means lye on their ſtomachs.

From a conſideration of the antiſeptic quality of both the ſal: marin: and of the vegetable acid, I was induced to make trial of their effects united in the manner above mentioned. It acted like a charm, and I find that from the uſe of it, the frequency of ſtools, gripes and teneſmus, have ſoon worn off; the ſtools gradually become of a natural conſiſtence and quantity; the ſpirits, ſtrength and appetite returned, and the patient has been reſtored to perfect health in a very few days.

When the dyſentery was of long ſtanding, ſtarch clyſters, with a ſmall portion of opium, abated the teneſmus.

This medicine was equally ſerviceable in diarrhœas.

DIABETES.

As I had ſucceeded ſo well in the cure of dyſenteries, I was determined to try its effects in the diabetes: ſeveral opportunities ſoon offered; but as theſe caſes were accompanied with other complaints, eſpecially with fevers of the remitting kind, it will be proper firſt to ſpeak of

THE REMITTENT FEVER.

This by far the moſt common fever within the tropics, is the leaſt underſtood, and conſequently for the moſt part badly treated. Strangers, who walk much, or work hard in the heat of the ſun, are more ſubject to it than ſeaſoned Europeans or natives of the country.

Dr. Cleghorn's deſcription of this fever is accurate and juſt. His method of cure, ſimple and eaſy. Every phyſician

ſician, who would wiſh to practice with ſucceſs, ſhould be well acquainted with that valuable performance, as alſo with what Dr. Lind has ſaid on the ſubject.

It is then ſufficient here, to obſerve that remittent fevers are often attended with diarrhœas, the diabetes, and ſometimes with a copious diſcharge of ſaliva, as if mercury had been previouſly given. In ſuch circumſtances I never found the bark of ſervice; a few glaſſes of the above mixture fully anſwered the intention, not only by removing theſe ſymptoms, but the fever at the ſame time.

The Peruvian bark afterwards, taken out of ſome of the ſame mixture, effectually ſecured the patient from a return of this dangerous malady.

The mixture rarely acted as an aſtringent in this or any other diſorder. But when this effect took place, the interpoſition of ſome lenient purge was deemed neceſſary.

BELLY-ACH.

The belly-ach with inflammatory ſymptoms has frequently occured in the courſe of my practice; they yielded with difficulty to bleeding, ſmall doſes of emetic tartar, a mercurial pill, repeated doſes of caſtor oil, diluting drinks, with nitre, fomentations and glyſters. A copious diſcharge of fœtid excrement for the moſt part gives immediate relief.

I have obſerved in many caſes, after moſt excruciating bellya-chs, that the ſtools were liquid, white, ſmall in quantity, and very fœtid. The patients being worn out with pain, grew diſpondent, did not care to ſpeak, fell into cold clammy ſweats and were very reſtleſs. They complained of an ill taſte in their mouths. Their tongues were much furred. Their breath offenſive, and they had a great propenſity to vomit.

Formerly I attempted the relief of thoſe threatening ſymptoms with the bark, in various forms, as well as

claret, and often ſaved my patient; ſometimes however I failed of ſucceſs. When ſuch caſes fall now under my care, I have immediate recourſe to the antiſeptic mixture; nor have I been hitherto diſappointed: the ſtools becoming leſs frequent on the uſe of it, and of a better conſiſtence; the cold ſweats alſo diſappear, and the ſpirits ſoon return, together with an appetite for food.

THE PUTRID SORE THROAT.

In June 1770, the putrid ſore throat made conſiderable havock amongſt adults and children. It attacked thoſe of a lax habit, who for a few days had ſlight head-achs, chillineſs and heats alternately, and an uneaſineſs about their throats, but not ſo much as to hinder their ſwallowing.

On examination, the mouth, tongue and gums were foul and ſlimy; the tonſils and uvula covered with white ſpecks or ſloughs; the breath was hot and offenſive; the ſkin felt hot and pungent to the touch; the pulſe low and quick; a diarrhœa often attended, and the patients were in general much dejected.

Antimonial wine with cordials and nouriſhing diet ſucceeded beſt, till the ſloughs or ſpots were removed and ſeparated; then the bark completed the cure. When a diarrhœa accompanied this diſorder, I gave the mixture with ſucceſs.

In all diſorders where a gargle is neceſſary, I make uſe of the above mixture in preference to any other; and I find it ſpeedily cleanſes the tongue, gums and fauces, and ſweetens the breath.

Where lemons or limes cannot be had, vinegar or cremor tartar may be ſubſtituted in their room.

From what has been ſaid, it is evident, that the medicine is poſſeſſed of conſiderable antiſeptic powers, and its virtue

virtue conſiſts in correcting the peccant matter in the ſtomach and inteſtinal canal.

All the diſeaſes in which I have given it, had a putrid tendency. I ſhall be happy to hear of its ſucceſs in your weſtern hemiſphere.

And am, with eſteem,
SIR,
Your moſt humble ſervant,
WILLIAM WRIGHT.

N° XXXVII.

Medical Hiſtory of the Cortex Ruber, or Red Bark; communicated to JOHN MORGAN, *M. D. Profeſſor of the Theory and Practice of Phyſic at Philadelphia, and F. R. S. London, &c.*

Read Feb. 20, 1784. I HAVE lately received the following communications upon the Cortex Ruber, which I have found ſo efficacious in the cure of obſtinate remittent and bilious fevers, that I think it my duty to lay them before this ſociety, in hopes of ſo valuable a medicine being thereby better known, and introduced more generally into practice.

Extract of a Letter from Thomas S. Duché, *dated London, Auguſt* 9, 1783.

" I was lately at a lecture delivered at Guy's hoſpital, by Dr. Saunders, upon the cure of intermittent fevers, and obſerving the doctor ſpoke very much in favour of a new ſpecies of bark which he had introduced into the practice of phyſic, I procured a ſpecimen of it for you, thinking,

thinking it might be agreeable to you to hear of any new improvements in the healing art. It is called *Red Bark*. According to his account it poſſeſſes ſo much virtue, and is of ſuch certain efficacy, that, compared with it, the common bark is an inert maſs. It contains a much larger portion of reſin, has a much ſtronger aromatic taſte than the common bark, and does not require half the quantity for a doſe. Amongſt other particulars, he mentioned the following proof of its ſuperior virtue, namely, that of this medicine, when adminiſtered in a ſimple cold infuſion, any given quantity is much ſtronger and effectual to remove the fever than a chemical extract from the ſame quantity of the other. I now ſend you a ſpecimen, by which you will be able to make a trial and form ſome judgment of its virtues."

T. S. DUCHÉ.

Soon after the receipt of the foregoing letter, I received the following valuable communication from Dr. George Davidſon of St. Lucia, which it affords me great pleaſure to lay before this ſociety.

St. Lucia, Auguſt 29, 1783.

To Doctor JOHN MORGAN, at Philadelphia.

SIR,

IF the ſubject upon which I have the honour to write you, ſhould be found to merit attention, and prove in any reſpect uſeful and advantageous to mankind, I ſhall eaſily ſtand excuſed in addreſſing you, perſonally unacquainted as I am.

I have by this opportunity ſent a ſmall ſpecimen of the Cinchona of this iſland, reſembling the Peruvian bark in its botanical character, and from the trial made here ſurpaſſing it in medical virtues. It is now nearly four years ſince the Caribæan bark was diſcovered upon the heights adjoining

adjoining Morne Fortuné, and introduced into practice by Dr. Young, phyſician to his Britannic majeſty's troops. The freſhneſs of the bark, the little attention beſtowed in drying it, and the large doſes in which it was exhibited, produced alarming fits of vomiting and purging, and deterred us, at that time, from the further proſecution of the ſubject, until the other day that a treatiſe upon the red bark, by Dr. Saunders of London, and a belief which we entertained that this was the ſame bark which he deſcribes, induced us again to make a trial of it. Having properly dried it, and given it in the cold infuſion with greater caution and in leſs doſes than at the firſt eſſay, we are now happy in aſſuring the public, that in moſt inſtances it has not diſappointed us. Still, however, notwithſtanding the utmoſt care in drying it, in ſome caſes it ſtill ſeems to retain its emetic and purgative qualities, as the ſtomach and firſt paſſages, in complaints here, are loaded with a quantity of putrid bile. Theſe are not its leaſt valuable properties. It will, however, be neceſſary when theſe effects are produced, to check them afterwards by opiates.

With regard to its preparations: I have generally given it in the cold infuſion either made with lime or cinnamon water. An extract made with ſpirits and water ſits eaſily on the ſtomach and can be given in larger quantities.

In ſome late caſes of tertians, where I have been called to the patient during the ſecond fit; without watching for its going off, I have begun with this bark, which effectually cleanſed the ſtomach and bowels, and paved the way for its future adminiſtration.

In putrid dyſenteries, and in a remarkable ſpecies of dyſentery, conjoined with an intermittent fever, which I have met with here, the bark has done more than all the remedies which I have ſeen employed. The purgative effects which it produced enabled us to throw it in earlier; the hardened ſcybula, the ſupport of the diſeaſe, were removed.

removed, the ſtomach and bowels braced up, and, by the interpoſition of opiates, the ſpaſms were removed.

Having ſent ſeveral ſpecimens of the bark for a trial to different parts of the continent of America, and particularly to my worthy friend Doctor Hall of Peterſburgh Virginia, I impatiently wait the reſult of your trials, and will eſteem myſelf particularly obliged by your communication. If you chuſe, I ſhall ſend you ſome of the young trees planted in tubs, with ſome of the ſeeds.

Should it be found to anſwer my expectation, the pleaſure reſulting from the thoughts of having communicated ſomething uſeful, will be to me ample enough recompenſe. I have the honor to be,

With the utmoſt reſpect,
Your moſt obedient humble ſervant,
GEORGE DAVIDSON.

P. S. Dr. Wright of Jamaica (in fifth vol. of medical commentaries,) deſcribes a ſpecies of Cinchona, with only one flower on a footſtalk; the ſame was likewiſe found at the Havanna. It differs in that particular from the old bark, which reſembles the St. Lucia bark, in having ſeveral flowers on each footſtalk.

The following is a Deſcription of the CINCHONA CARIBÆA SANCTÆ LUCIÆ.

The tree is commonly found in ravines, near ſprings, under the ſhade of a larger tree. It delights in places well ſhaded, and defended from the north-eaſt trade wind. The ſoil is commonly a ſtiff red earth with a clayey ſubſtratum; quantities of ſmall beautiful chryſtals, of a regular angular form, are found intermixed.

The tree is about the ſize of the cherry tree; ſeldom exceeding the thickneſs of the thigh, and twenty-five feet in height.

The

The flowers begin to appear at the commencement of the rainy ſeaſon in beautiful tufts, upon pannicles branched out in threes and fours. I have never ſeen that ſpecies deſcribed by Jacquin and found at the Havannah, *pedunculis unifloris*.

Before the corolla is fully expanded, and the ſtamina make their appearance without the tube of the corolla, the flower is white, but it afterwards turns to a beautiful purple. Then dropping off, the germen enlarges to the ſize of an hazle-nut, oblong and round. It gradually dries, burſts in two, and ſcatters the ſeeds, which fall to the ground and again take root.

The wood of the tree is light, ſpongy, and fit for no uſeful purpoſe. It has not the bitter taſte of the bark. The leaves are very bitter, and the flowers, ſeeds, &c. ſeem to poſſeſs the bitterneſs and aſtringency in a more eminent degree.

An ounce of the bark in fine powder infuſed in a quart of cold water for twenty-four hours, and the infuſion afterwards filtered, appears higher coloured than a decoction made with double the quantity of the old bark. The colour which it ſtrikes with the *tinct. flor. martial.* and *ſal martis*, is likewiſe of a deeper black. The ſpirituous tincture is of a deep red colour, and ſtrikes a deep black by the addition of the preparations of iron.

The taſte of the Cinchona Caribæa is manifeſtly more aſtringent than the taſte of the old bark; an inference may therefore, *à priori*, be made, that its tonic powers are greater.

The quantity of reſin which it yields is much more conſiderable, and an extract made with both ſpirits and water, ſeems to poſſeſs the whole virtues of the bark.

N° XXXVIII.

A Letter from Dr. BENJAMIN FRANKLIN, *to* Mr. ALPHONSUS le ROY, *Member of ſeveral Academies, at Paris. Containing ſundry Maritime Obſervations.*

At Sea, on board the London Packet, Capt. Truxton, Auguſt 1785.

SIR,

Read Dec. 2, 1785. YOUR learned writings on the navigation of the antients, which contain a great deal of curious information; and your very ingenious contrivances for improving the modern ſails (*voilure*) of which I ſaw with great pleaſure a ſucceſsful trial on the river Seine, have induced me to ſubmit to your conſideration and judgment, ſome thoughts I have had on the latter ſubject.

Thoſe mathematicians who have endeavoured to improve the ſwiftneſs of veſſels, by calculating to find the form of leaſt reſiſtance, ſeem to have conſidered a ſhip as a body moving through one fluid only, the water; and to have given little attention to the circumſtance of her moving through another fluid, the air. It is true that when a veſſel ſails right before the wind, this circumſtance is of no importance, becauſe the wind goes with her; but in every deviation from that courſe, the reſiſtance of the air is ſomething, and becomes greater in proportion as that deviation increaſes. I wave at preſent the conſideration of thoſe different degrees of reſiſtance given by the air to that part of the hull which is above water, and confine myſelf to that given to the ſails; for their motion through the air is reſiſted by the air, as the motion of the hull through the water is reſiſted by the water, though with leſs force as the air is a lighter fluid. And to ſimplify the diſcuſſion as much as poſſible, I would ſtate one ſituation only, to wit, that of the wind upon the beam, the ſhip's courſe being directly acroſs the wind; and I would ſuppoſe

ſuppoſe the ſail ſet in an angle of 45 degrees with the keel, as in the following figure; wherein

A B repreſents the body of the veſſel, C D the poſition of the ſail, E E E the direction of the wind, M M the line of motion. In obſerving this figure it will appear, that ſo much of the body of the veſſel as is immerſed in the water, muſt, to go forward, remove out of its way what water it meets with between the pricked lines F F. And the ſail, to go forward, muſt move out of its way all the air its whole dimenſion meets with between the pricked lines C G and D G. Thus both the fluids give reſiſtance to the motion, each in proportion to the quantity of matter contained in the dimenſion to be removed. And though the air is vaſtly lighter than the water, and therefore more eaſily removed, yet the dimenſion being much greater its effect is very conſiderable. Plate IV. Figure 1.

It is true that in the caſe ſtated, the reſiſtance given by the air between thoſe lines to the motion of the ſail is not apparent to the eye, becauſe the greater force of the wind which ſtrikes it in the direction E E E, overpowers its effect, and keeps the ſail full in the curve a, a, a, a, a. But ſuppoſe the wind to ceaſe, and the veſſel in a calm to be impelled with the ſame ſwiftneſs by oars, the ſail would then appear filled in the contrary curve b, b, b, b, b, when prudent men would immediately perceive that the air reſiſted its motion, and would order it to be taken in.

Is there any poſſible means of diminiſhing this reſiſtance, while the ſame quantity of ſail is expoſed to the action of the wind, and therefore the ſame force obtained from it? I think there is, and that it may be done by dividing the ſail into a number of parts, and placing thoſe parts in a line one behind the other; thus inſtead of one ſail extending from C to D, figure 2, if four ſails containing together the ſame quantity of canvas, were placed as in figure 3, each having one quarter of the di-

menſions of the great ſail, and expoſing a quarter of its ſurface to the wind, would give a quarter of the force; ſo that the whole force obtained from the wind would be the ſame, while the reſiſtance from the air would be nearly reduced to the ſpace between the pricked lines *a b* and *c d*, before the foremoſt ſail.

It may perhaps be doubted whether the reſiſtance from the air would be ſo diminiſhed; ſince poſſibly each of the following ſmall ſails having alſo air before it, which muſt be removed, the reſiſtance on the whole would be the ſame.

This is then a matter to be determined by experiment. I will mention one that I many years ſince made with ſucceſs for another purpoſe; and I will propoſe another ſmall one eaſily made. If that too ſucceeds, I ſhould think it worth while to make a larger, though at ſome expence, on a river boat; and perhaps time and the improvements experience will afford, may make it applicable with advantage to larger veſſels.

Having near my kitchen chimney a round hole of eight inches diameter, through which was a conſtant ſteady current of air, increaſing or diminiſhing only as the fire increaſed or diminiſhed, I contrived to place my jack ſo as to receive that current; and taking off the flyers, I fixed in their ſtead on the ſame pivot a round tin plate of near the ſame diameter with the hole; and having cut it in radial lines almoſt to the centre, ſo as to have ſix equal vanes, I gave to each of them the obliquity of forty-five degrees. They moved round, without the weight, by the impreſſion only of the current of air, but too ſlowly for the purpoſe of roaſting. I ſuſpected that the air ſtruck by the back of each vane might poſſibly by its reſiſtance retard the motion; and to try this, I cut each of them into two, and I placed the twelve, each having the ſame obliquity, in a line behind each other, when I perceived a great augmentation in its velocity, which encouraged me to divide

vide them once more, and, continuing the ſame obliquity, I placed the twenty-four behind each other in a line, when the force of the wind being the ſame, and the ſurface of vane the ſame, they moved round with much greater rapidity, and perfectly anſwered my purpoſe.

The ſecond experiment that I propoſe, is, to take two playing cards of the ſame dimenſions, and cut one of them tranſverſely into eight equal pieces; then with a needle ſtring them upon two threads one near each end, and place them ſo upon the threads that, when hung up, they may be one exactly over the other, at a diſtance equal to their breadth, each in a horizontal poſition; and let a ſmall weight, ſuch as a bird-ſhot, be hung under them, to make them fall in a ſtraight line when let looſe. Suſpend alſo the whole card by threads from its four corners, and hang to it an equal weight, ſo as to draw it downwards when let fall, its whole breadth preſſing againſt the air. Let thoſe two bodies be attached, one of them to one end of a thread a yard long, the other to the other end. Extend a twine under the ceiling of a room, and put through it at thirty inches diſtance two pins bent in the form of fiſh-hooks. On theſe two hooks hang the two bodies, the thread that connects them extending parallel to the twine, which thread being cut, they muſt begin to fall at the ſame inſtant. If they take equal time in falling to the floor, it is a proof that the reſiſtance of the air is in both caſes equal. If the whole card requires a longer time, it ſhows that the ſum of the reſiſtances to the pieces of the cut card is not equal to the reſiſtance of the whole one*.

This principle ſo far confirmed, I would proceed to make a larger experiment, with a ſhallop, which I would rig in this manner.

A B is

* The motion of the veſſel made it inconvenient to try this ſimple experiment, at ſea, when the propoſal of it was written. But it has been tried ſince we came on ſhore, and ſucceeded as the other.

Plate IV. Figure 4. A B is a long boom, from which are hoiſted ſeven jibs, a, b, c, d, e, f, g, each a ſeventh part of the whole dimenſions, and as much more as will fill the whole ſpace when ſet in an angle of forty-five degrees, ſo that they may lap when going before the wind, and hold more wind when going large. Thus rigged, when going right before the wind, the boom ſhould be brought at right angles with the keel, by means of the ſheet ropes C D, and all the ſails hauled flat to the boom.

Theſe poſitions of boom and ſails to be varied as the wind quarters. But when the wind is on the beam, or when you would turn to windward, the boom is to be hauled right fore and aft, and the ſails trimmed according as the wind is more or leſs againſt your courſe.

It ſeems to me that the management of a ſhallop ſo rigged would be very eaſy, the ſails being run up and down ſeparately, ſo that more or leſs ſail may be made at pleaſure; and I imagine, that there being full as much ſail expoſed to the force of the wind which impells the veſſel in its courſe, as if the whole were in one piece, and the reſiſtance of the dead air againſt the foreſide of the ſail being diminiſhed, the advantage of ſwiftneſs would be very conſiderable; beſides that the veſſel would lie nearer the wind.

Since we are on the ſubject of improvements in navigation, permit me to detain you a little longer with a ſmall relative obſervation. Being, in one of my voyages, with ten merchant-ſhips under convoy of a frigate at anchor in Torbay, waiting for a wind to go to the weſtward; it came fair, but brought in with it a conſiderable ſwell. A ſignal was given for weighing, and we put to ſea all together; but three of the ſhips left their anchors, their cables parting juſt as the anchors came a-peak. Our cable held, and we got up our anchor; but the ſhocks the ſhip felt before the anchor got looſe from the ground, made me reflect on what might poſſibly have cauſed the breaking

ing of the other cables; and I imagined it might be the ſhort bending of the cable juſt without the hauſe-hole, from a horizontal to an almoſt vertical poſition, and the ſudden violent jerk it receives by the riſing of the head of the ſhip on the ſwell of a wave while in that poſition. For example, ſuppoſe a veſſel hove up ſo as to have her head nearly over her anchor, which ſtill keeps its hold, perhaps in a tough bottom; if it were calm, the cable ſtill out would form nearly a perpendicular line, meaſuring the diſtance between the hauſe-hole and the anchor; but if there is a ſwell, her head in the trough of the ſea will fall below the level, and when lifted on the wave will be as much above it. In the firſt caſe the cable will hang looſe and bend perhaps as in figure 5. In the ſecond caſe figure 6, the cable will be drawn ſtraight with a jerk, muſt ſuſtain the whole force of the riſing ſhip, and muſt either looſen the anchor, reſiſt the riſing force of the ſhip, or break. But why does it break at the hauſe-hole?

Let us ſuppoſe it a cable of three inches diameter, and repreſented by figure 7. If this cable is to be bent round the corner A, it is evident that either the part of the triangle contained between the letters a, b, c, muſt ſtretch conſiderably, and thoſe moſt that are neareſt the ſurface; or that the parts between d, e, f, muſt be compreſſed; or both, which moſt probably happens. In this caſe the lower half of the thickneſs affords no ſtrength againſt the jerk, it not being ſtrained, the upper half bears the whole, and the yarns near the upper ſurface being firſt and moſt ſtrained, break firſt, and the next yarns follow; for in this bent ſituation they cannot bear the ſtrain all together, and each contribute its ſtrength to the whole, as they do when the cable is ſtrained in a ſtraight line.

To remedy this, methinks it would be well to have a kind of large pulley wheel, fixed in the hauſe-hole, ſuppoſe of two feet diameter, over which the cable might paſs; and being there bent gradually to the round of the wheel,

wheel, would thereby be more equally ſtrained, and better able to bear the jerk, which may ſave the anchor, and by that means in the courſe of the voyage may happen to ſave the ſhip.

One maritime obſervation more ſhall finiſh this letter. I have been a reader of news-papers now near ſeventy years, and I think few years paſs without an account of ſome veſſel met with at ſea, with no ſoul living on board, and ſo many feet of water in her hold, which veſſel has nevertheleſs been ſaved and brought into port: and when not met with at ſea, ſuch forſaken veſſels have often come aſhore on ſome coaſt. The crews who have taken to their boats and thus abandoned ſuch veſſels, are ſometimes met with and taken up at ſea by other ſhips, ſometimes reach a coaſt, and are ſometimes never heard of. Thoſe that give an account of quitting their veſſels, generally ſay, that ſhe ſprung a leak, that they pumped for ſome time, that the water continued to riſe upon them, and that deſpairing to ſave her, they had quitted her leſt they ſhould go down with her. It ſeems by the event that this fear was not always well founded, and I have endeavoured to gueſs at the reaſon of the people's too haſty diſcouragement.

When a veſſel ſprings a leak near her bottom, the water enters with all the force given by the weight of the column of water, without, which force is in proportion to the difference of level between the water without and that within. It enters therefore with more force at firſt, and in greater quantity, than it can afterwards when the water within is higher. The bottom of the veſſel too is narrower, ſo that the ſame quantity of water coming into that narrow part, riſes faſter than when the ſpace for it to flow in is larger. This helps to terrify. But as the quantity entering is leſs and leſs as the ſurfaces without and within become more nearly equal in height, the pumps that could not keep the water from riſing at firſt, might afterwards be able to prevent its riſing higher, and the people might

might have remained on board in ſafety, without hazarding themſelves in an open boat on the wide ocean. (Fig. 8.)

Beſides the greater equality in the height of the two ſurfaces, there may ſometimes be other cauſes that retard the farther ſinking of a leaky veſſel. The riſing water within may arrive at quantities of light wooden work, empty cheſts, and particularly empty water caſks, which if fixed ſo as not to float themſelves may help to ſuſtain her. Many bodies which compoſe a ſhip's cargo may be ſpecifically lighter than water, all theſe when out of water are an additional weight to that of the ſhip, and ſhe is in proportion preſſed deeper into the water; but as ſoon as theſe bodies are immerſed, they weigh no longer on the ſhip, but on the contrary, if fixed, they help to ſupport her, in proportion as they are ſpecifically lighter than the water. And it ſhould be remembered, that the largeſt body of a ſhip may be ſo balanced in the water, that an ounce leſs or more of weight may leave her at the ſurface or ſink her to the bottom. There are alſo certain heavy cargoes, that when the water gets at them are continually diſſolving, and thereby lightening the veſſel, ſuch as ſalt and ſugar. And as to water caſks mentioned above, ſince the quantity of them muſt be great in ſhips of war where the number of men conſume a great deal of water every day, if it had been made a conſtant rule to bung them up as faſt as they were emptied, and to diſpoſe the empty caſks in proper ſituations, I am perſuaded that many ſhips which have been ſunk in engagements, or have gone down afterwards, might with the unhappy people have been ſaved; as well as many of thoſe which in the laſt war foundered, and were never heard of. While on this topic of ſinking, one cannot help recollecting the well known practice of the Chineſe, to divide the hold of a great ſhip into a number of ſeparate chambers by partitions tight caulked, (of which you gave a model in your boat upon the Seine) ſo that if a leak ſhould ſpring in one of them

the others are not affected by it; and though that chamber ſhould fill to a level with the ſea, it would not be ſufficient to ſink the veſſel. We have not imitated this practice. Some little diſadvantage it might occaſion in the ſtowage is perhaps one reaſon, though that I think might be more than compenſated by an abatement in the inſurance that would be reaſonable, and by a higher price taken of paſſengers, who would rather prefer going in ſuch a veſſel. But our ſeafaring people are brave, deſpiſe danger, and reject ſuch precautions of ſafety, being cowards only in one ſenſe, that of *fearing* to be *thought afraid.*

I promiſed to finiſh my letter with the laſt obſervation, but the garrulity of the old man has got hold of me, and as I may never have another occaſion of writing on this ſubject, I think I may as well now, once for all, empty my nautical budget, and give you all the thoughts that have in my various long voyages occurred to me relating to navigation. I am ſure that in you they will meet with a candid judge, who will excuſe my miſtakes on account of my good intention.

There are ſix accidents that may occaſion the loſs of ſhips at ſea. We have conſidered one of them, that of foundering by a leak. The other five are, 1. Overſetting by ſudden flaws of wind, or by carrying ſail beyond the bearing. 2. Fire by accident or careleſſneſs. 3. A heavy ſtroke of lightning, making a breach in the ſhip, or firing the powder. 4. Meeting and ſhocking with other ſhips in the night. 5. Meeting in the night with iſlands of ice.

To that of overſetting, privateers in their firſt cruize have, as far as has fallen within my knowledge or information, been more ſubject than any other kind of veſſels. The double deſire of being able to overtake a weaker flying enemy, or to eſcape when purſued by a ſtronger, has induced the owners to overmaſt their cruizers, and to ſpread too much canvas; and the great number of men, many of them

them not ſeamen, who being upon deck when a ſhip heels ſuddenly are huddled down to leeward, and increaſe by their weight the effect of the wind. This therefore ſhould be more attended to and guarded againſt, eſpecially as the advantage of lofty maſts is problematical. For the upper ſails have greater power to lay a veſſel more on her ſide, which is not the moſt advantageous poſition for going ſwiftly through the water. And hence it is that veſſels which have loſt their lofty maſts, and been able to make little more ſail afterwards than permitted the ſhip to ſail upon an even keel, have made ſo much way, even under jury maſts, as to ſurprize the mariners themſelves. But there is beſides, ſomething in the modern form of our ſhips that ſeems as if calculated expreſsly to allow their overſetting more eaſily. The ſides of a ſhip inſtead of ſpreading out as they formerly did in the upper works, are of late years turned in, ſo as to make the body nearly round, and more reſembling a caſk. I do not know what the advantages of this conſtruction are, except that ſuch ſhips are not ſo eaſily boarded. To me it ſeems a contrivance to have leſs room in a ſhip at nearly the ſame expence. For it is evident that the ſame timber and plank conſumed in raiſing the ſides from a to b, and from d to c, would have raiſed them from a to e, and from d to f, fig. 9. In this form all the ſpaces between e, a, b, and c, d, f, would have been gained, the deck would have been larger, the men would have had more room to act, and not have ſtood ſo thick in the way of the enemy's ſhot; and the veſſel the more ſhe was laid down on her ſide, the more bearing ſhe would meet with, and more effectual to ſupport her, as being farther from the center. Whereas in the preſent form, her ballaſt makes the chief part of her bearing, without which ſhe would turn in the ſea almoſt as eaſily as a barrel. More ballaſt by this means becomes neceſſary, and that ſinking a veſſel deeper in the water occaſions more reſiſtance to her going through it. The

Bermudian ſloops ſtill keep with advantage to the old ſpreading form. The iſlanders in the great Pacific ocean, though they have no large ſhips, are the moſt expert boat-ſailors in the world, navigating that ſea ſafely with their proas, which they prevent overſetting by various means. Their ſailing proas for this purpoſe have outriggers generally to windward, above the water, on which one or more men are placed to move occaſionally further from or nearer to the veſſel as the wind freſhens or ſlackens. But ſome have their outriggers to leeward, which reſting on the water ſupport the boat ſo as to keep her upright when preſſed down by the wind. Their boats moved by oars or rather by paddles, are, for long voyages, fixed two together by croſs bars of wood that keep them at ſome diſtance from each other, and ſo render their overſetting next to impoſſible. How far this may be practicable in larger veſſels, we have not yet ſufficient experience. I know of but one trial made in Europe, which was about one hundred years ſince by, Sir William Petty. He built a double veſſel, to ſerve as a pacquet boat between England and Ireland. Her model ſtill exiſts in the muſeum of the Royal Society, where I have ſeen it. By the accounts we have of her, ſhe anſwered well the purpoſe of her conſtruction, making ſeveral voyages; and though wrecked at laſt by a ſtorm, the misfortune did not appear owing to her particular conſtruction, ſince many other veſſels of the common form were wrecked at the ſame time. The advantage of ſuch a veſſel is: That ſhe needs no ballaſt, therefore ſwims either lighter or will carry more goods; and that paſſengers are not ſo much incommoded by her rolling: to which may be added, that if ſhe is to defend herſelf by her cannon, they will probably have more effect, being kept more generally in a horizontal poſition, than thoſe in common veſſels. I think however that it would be an improvement of that model, to make the ſides

which

which are oppoſed to each other perfectly parallel, though the other ſides are formed as in common thus, figure 10.

The building of a double ſhip would indeed be more expenſive in proportion to her burthen; and that perhaps is ſufficient to diſcourage the method.

The accident of fire is generally well guarded againſt by the prudent captain's ſtrict orders againſt ſmoking between decks, or carrying a candle there out of a lanthorn. But there is one dangerous practice which frequent terrible accidents have not yet been ſufficient to aboliſh; that of carrying ſtore-ſpirits to ſea in caſks. Two large ſhips, the Seraphis and the Duke of Athol, one an Eaſt-Indiaman, the other a frigate, have been burnt within theſe two laſt years, and many lives miſerably deſtroyed, by drawing ſpirits out of a caſk near a candle. It is high time to make it a general rule, that all the ſhip's ſtore of ſpirits ſhould be carried in bottles.

The misfortune by a ſtroke of lightning I have in my former writings endeavoured to ſhow a method of guarding againſt, by a chain and pointed rod, extending, when run up, from above the top of the maſt to the ſea. Theſe inſtruments are now made and ſold at a reaſonable price by *Nairne and Co.* in London, and there are ſeveral inſtances of ſucceſs attending the uſe of them. They are kept in a box, and may be run up and fixed in about five minutes, on the apparent approach of a thunder guſt.

Of the meeting and ſhocking with other ſhips in the night, I have known two inſtances in voyages between London and America. In one both ſhips arrived though much damaged, each reporting their belief that the other muſt have gone to the bottom. In the other, only one got to port; the other was never afterwards heard of. Theſe inſtances happened many years ago, when the commerce between Europe and America was not a tenth part of what it is at preſent, ſhips of courſe thinner ſcattered, and the chance of meeting proportionably leſs. It has long been

been the practice to keep a *look-out before* in the channel, but at ſea it has been neglected. If it is not at preſent thought worth while to take that precaution, it will in time become of more conſequence; ſince the number of ſhips at ſea is continually augmenting. A drum frequently beat or a bell rung in a dark night, might help to prevent ſuch accidents.

Iſlands of ice are frequently ſeen off the banks of Newfoundland, by ſhips going between North-America and Europe. In the day-time they are eaſily avoided, unleſs in a very thick fog. I remember two inſtances of ſhips running againſt them in the night. The firſt loſt her bowſprit, but received little other damage. The other ſtruck where the warmth of the ſea had waſted the ice next to it, and a part hung over above. This perhaps ſaved her, for ſhe was under great way; but the upper part of the cliff taking her foretopmaſt, broke the ſhock, though it carried away the maſt. She diſengaged herſelf with ſome difficulty, and got ſafe into port; but the accident ſhows the poſſibility of other ſhips being wrecked and ſunk by ſtriking thoſe vaſt maſſes of ice, of which I have ſeen one that we judged to be ſeventy feet high above the water, conſequently eight times as much under water; and it is another reaſon for keeping a good *look-out before*, though far from any coaſt that may threaten danger.

It is remarkable that the people we conſider as ſavages, have improved the art of ſailing- and rowing-boats in ſeveral points beyond what we can pretend to. We have no ſailing boats equal to the flying proas of the ſouth ſeas, no rowing or paddling boat equal to that of the Greenlanders for ſwiftneſs and ſafety. The birch canoes of the North-American Indians have alſo ſome advantageous properties. They are ſo light that two men may carry one of them over land, which is capable of carrying a dozen upon the water; and in heeling they are not ſo ſubject to take in water as our boats, the ſides of which are loweſt

loweſt in the middle where it is moſt likely to enter, this being higheſt in that part, as in figure 11.

The Chineſe are an enlightened people, the moſt antiently civilized of any exiſting, and their arts are antient, a preſumption in their favour: their method of rowing their boats differs from ours, the oars being worked either two a-ſtern as we ſcull, or on the ſides with the ſame kind of motion, being hung parallel to the keel on a rail and always acting in the water, not perpendicular to the ſide as ours are, nor lifted out at every ſtroke, which is a loſs of time, and the boat in the interval loſes motion. They ſee our manner, and we theirs, but neither are diſpoſed to learn of or copy the other.

To the ſeveral means of moving boats mentioned above, may be added the ſingular one lately exhibited at Javelle, on the Seine below Paris, where a clumſy boat was moved acroſs that river in three minutes by rowing, not in the water, but in the air, that is, by whirling round a ſet of windmill vanes fixed to a horizontal axis, parallel to the keel, and placed at the head of the boat. The axis was bent into an elbow at the end, by the help of which it was turned by one man at a time. I ſaw the operation at a diſtance. The four vanes appeared to be about five feet long, and perhaps two and a half wide. The weather was calm. The labour appeared to be great for one man, as the two ſeveral times relieved each other. But the action upon the air by the oblique ſurfaces of the vanes muſt have been conſiderable, as the motion of the boat appeared tolerably quick going and returning; and ſhe returned to the ſame place from whence ſhe firſt ſet out, notwithſtanding the current. This machine is ſince applied to the moving of air balloons: An inſtrument ſimilar may be contrived to move a boat by turning under water.

Several mechanical projectors have at different times propoſed to give motion to boats, and even to ſhips, by means of circular rowing, or paddles placed on the circumference:

ference of wheels to be turned conſtantly on each ſide of the veſſel; but this method, though frequently tried, has never been found ſo effectual as to encourage a continuance of the practice. I do not know that the reaſon has hitherto been given. Perhaps it may be this, that great part of the force employed contributes little to the motion. For inſtance, (fig. 12.) of the four paddles a, b, c, d, all under water, and turning to move a boat from X to Y, c has the moſt power, b nearly though not quite as much, their motion being nearly horizontal; but the force employed in moving a, is conſumed in preſſing almoſt downright upon the water till it comes to the place of b; and the force employed in moving d is conſumed in lifting the water till d arrives at the ſurface; by which means much of the labour is loſt. It is true, that by placing the wheels higher out of the water, this waſte labour will be diminiſhed in a calm, but where a ſea runs, the wheels muſt unavoidably be often dipt deep in the waves, and the turning of them thereby rendered very laborious to little purpoſe.

Among the various means of giving motion to a boat, that of M. Bernoulli appears one of the moſt ſingular, which was to have fixed in the boat a tube in the form of an L, the upright part to have a funnel-kind of opening at top, convenient for filling the tube with water; which deſcending and paſſing through the lower horizontal part, and iſſuing in the middle of the ſtern, but under the ſurface of the river, ſhould puſh the boat forward. There is no doubt that the force of the deſcending water would have a conſiderable effect, greater in proportion to the height from which it deſcended; but then it is to be conſidered, that every bucket-full pumped or dipped up into the boat, from its ſide or through its bottom, muſt have its *vis inertiæ* overcome ſo as to receive the motion of the boat, before it can come to give motion by its deſcent; and that will be a deduction from the moving power.

To

To remedy this, I would propoſe the addition of another ſuch L pipe, and that they ſhould ſtand back to back in the boat thus, figure 13. the forward one being worked as a pump, and ſucking in the water at the head of the boat, would draw it forward while puſhed in the ſame direction by the force at the ſtern. And after all it ſhould be calculated whether the labour of pumping would be leſs than that of rowing. A fire-engine might poſſibly in ſome caſes be applied in this operation with advantage.

Perhaps this labour of raiſing water might be ſpared, and the whole force of a man applied to the moving of a boat by the uſe of air inſtead of water; ſuppoſe the boat conſtructed in this form, figure 14. A, a tube round or ſquare of two feet diameter, in which a piſton may move up and down. The piſton to have valves in it, opening inwards to admit air when the piſton riſes; and ſhutting, when it is forced down by means of the lever B turning on the center C. The tube to have a valve D, to open when the piſton is forced down, and let the air paſs out at E, which ſtriking forcibly againſt the water abaft muſt puſh the boat forward. If there is added an air-veſſel F properly valved and placed, the force would continue to act while a freſh ſtroke is taken with the lever. The boatman might ſtand with his back to the ſtern, and putting his hands behind him, work the motion by taking hold of the croſs bar at B, while another ſhould ſteer; or if he had two ſuch pumps, one on each ſide of the ſtern, with a lever for each hand, he might ſteer himſelf by working occaſionally more or harder with either hand, as watermen now do with a pair of ſculls. There is no poſition in which the body of a man can exert more ſtrength than in pulling right upwards.

To obtain more ſwiftneſs, greaſing the bottom of a veſſel is ſometimes uſed, and with good effect. I do not know that any writer has hitherto attempted to explain this. At firſt ſight one would imagine, that though the friction of a hard body ſliding on another hard body, and

the reſiſtance occaſioned by that friction, might be diminiſhed by putting greaſe between them, yet that a body ſliding on a fluid, ſuch as water, ſhould have no need of nor receive any advantage from ſuch greaſing. But the fact is not diſputed. And the reaſon perhaps may be this. The particles of water have a mutual attraction, called the attraction of adheſion. Water alſo adheres to wood, and to many other ſubſtances, but not to greaſe: On the contrary they have a mutual repulſion, ſo that it is a queſtion whether when oil is poured on water, they ever actually touch each other; for a drop of oil upon water, inſtead of ſticking to the ſpot where it falls, as it would if it fell on a looking-glaſs, ſpreads inſtantly to an immenſe diſtance in a film extremely thin, which it could not eaſily do if it touched and rubbed or adhered even in a ſmall degree to the ſurface of the water. Now the adheſive force of water to itſelf, and to other ſubſtances, may be eſtimated from the weight of it neceſſary to ſeparate a drop, which adheres, while growing, till it has weight enough to force the ſeparation and break the drop off. Let us ſuppoſe the drop to be the ſize of a pea, then there will be as many of theſe adheſions as there are drops of that ſize touching the bottom of a veſſel, and theſe muſt be broken by the moving power, every ſtep of her motion that amounts to a drop's breadth: And there being no ſuch adheſions to break between the water and a greaſed bottom, may occaſion the difference.

So much reſpecting the motion of veſſels. But we have ſometimes occaſion to ſtop their motion; and if a bottom is near enough we can caſt anchor: Where there are no ſoundings, we have as yet no means to prevent driving in a ſtorm, but by lying-to, which ſtill permits driving at the rate of about two miles an hour; ſo that in a ſtorm continuing fifty hours, which is not an uncommon caſe, the ſhip may drive one hundred miles out of her courſe; and ſhould ſhe in that diſtance meet with a lee ſhore, ſhe may be loſt.

To

To prevent this driving to leeward in deep water, a ſwimming anchor is wanting, which ought to have theſe properties.

1. It ſhould have a ſurface ſo large as being at the end of a hauſer in the water, and placed perpendicularly, ſhould hold ſo much of it, as to bring the ſhip's head to the wind, in which ſituation the wind has leaſt power to drive her.

2. It ſhould be able by its reſiſtance to prevent the ſhip's receiving way.

3. It ſhould be capable of being ſituated below the heave of the ſea, but not below the undertow.

4. It ſhould not take up much room in the ſhip.

5. It ſhould be eaſily thrown out, and put into its proper ſituation.

6. It ſhould be eaſy to take in again, and ſtow away.

An ingenious old mariner whom I formerly knew, propoſed as a ſwimming anchor for a large ſhip to have a ſtem of wood twenty-five feet long and four inches ſquare, with four boards of 18, 16, 14, and 12, feet long, and one foot wide, the boards to have their ſubſtance thickened ſeveral inches in the middle by additional wood, and to have each a four inch ſquare hole through its middle, to permit its being ſlipt on occaſionally upon the ſtem, and at right angles with it; where all being placed and fixed at four feet diſtance from each other, it would have the appearance of the old mathematical inſtrument called a foreſtaff. This thrown into the ſea, and held by a hauſer veered out to ſome length, he conceived would bring a veſſel up, and prevent her driving, and when taken in might be ſtowed away by ſeparating the boards from the ſtem. Figure 15. Probably ſuch a ſwimming anchor would have ſome good effect, but it is ſubject to this objection, that lying on the ſurface of the ſea, it is liable to be hove forward by every wave, and thereby give ſo much leave for the ſhip to drive.

Two machines for this purpoſe have occurred to me, which though not ſo ſimple as the above, I imagine would be more effectual, and more eaſily manageable. I will endeavour to deſcribe them, that they may be ſubmitted to your judgment, whether either would be ſerviceable; and if they would, to which we ſhould give the preference.

The firſt is to be formed, and to be uſed in the water on almoſt the ſame principles with thoſe of a paper kite uſed in the air. Only as the paper kite riſes in the air, this is to deſcend in the water. Its dimenſions will be different for ſhips of different ſize.

To make one of ſuppoſe fifteen feet high; take a ſmall ſpar of that length for the back-bone, AB, figure 16, a ſmaller of half that length CD, for the croſs piece. Let theſe be united by a bolt at E, yet ſo as that by turning on the bolt they may be laid parallel to each other. Then make a ſail of ſtrong canvas, in the ſhape of figure 17. To form this, without waſte of ſail-cloth, ſew together pieces of the proper length, and for half the breadth, as in figure 18, then cut the whole in the diagonal lines a, b, c, and turn the piece F ſo as to place its broad part oppoſite to that of the piece G, and the piece H in like manner oppoſite to I, which when all ſewed together will appear as in figure 17. This ſail is to be extended on the croſs of figure 16, the top and bottom points well ſecured to the ends of the long ſpar; the two ſide points d, e, faſtened to the ends of two cords, which coming from the angle of the loop (which muſt be ſimilar to the loop of a kite) paſs through two rings at the ends of the ſhort ſpar, ſo as that on pulling upon the loop the ſail will be drawn to its extent. The whole may, when aboard, be furled up, as in figure 19, having a rope from its broad end, to which is tied a bag of ballaſt for keeping that end downwards when in the water, and at the other end another rope with an empty keg at its end to float on the ſurface; this

this rope long enough to permit the kite's deſcending into the undertow, or if you pleaſe lower into ſtill water. It ſhould be held by a hauſer. To get it home eaſily, a ſmall looſe rope may be veered out with it, fixed to the keg. Hauling on that rope will bring the kite home with ſmall force, the reſiſtance being ſmall as it will then come endways.

It ſeems probable that ſuch a kite at the end of a long hauſer would keep a ſhip with her head to the wind, and reſiſting every tug, would prevent her driving ſo faſt as when her ſide is expoſed to it, and nothing to hold her back. If only half the driving is prevented, ſo as that ſhe moves but fifty miles inſtead of the hundred during a ſtorm, it may be ſome advantage, both in holding ſo much diſtance as is ſaved, and in keeping from a lee ſhore. If ſingle canvas ſhould not be found ſtrong enough to bear the tug without ſplitting, it may be doubled, or ſtrengthened by a netting behind it, repreſented by figure 20.

The other machine for the ſame purpoſe, is to be made more in the form of an umbrella, as repreſented, figure 21. The ſtem of the umbrella a ſquare ſpar of proper length, with four moveable arms, of which two are repreſented C, C, figure 22. Theſe arms to be fixed in four joint cleats, as D, D, &c. one on each ſide of the ſpar, but ſo as that the four arms may open by turning on a pin in the joint. When open they form a croſs, on which a four ſquare canvas ſail is to be extended, its corners faſtened to the ends of the four arms. Thoſe ends are alſo to be ſtayed by ropes faſtened to the ſtem or ſpar, ſo as to keep them ſhort of being at right angles with it: And to the end of one of the arms ſhould be hung the ſmall bag of ballaſt, and to the end of the oppoſite arm the empty keg. This on being thrown into the ſea, would immediately open; and when it had performed its function, and the ſtorm over, a ſmall rope from its other end being pulled on, would turn it, cloſe it, and draw it eaſily home to the ſhip. This machine ſeems more ſimple in its operation,

ration, and more eaſily manageable than the firſt, and perhaps may be as effectual.*

Veſſels are ſometimes retarded, and ſometimes forwarded in their voyages, by currents at ſea, which are often not perceived. About the year 1769 or 70, there was an application made by the board of cuſtoms at Boſton, to the lords of the treaſury in London, complaining that the packets between Falmouth and New-York, were generally a fortnight longer in their paſſages, than merchant ſhips from London to Rhode-Iſland, and propoſing that for the future they ſhould be ordered to Rhode-Iſland inſtead of New-York. Being then concerned in the management of the American poſt-office, I happened to be conſulted on the occaſion; and it appearing ſtrange to me that there ſhould be ſuch a difference between two places, ſcarce a day's run aſunder, eſpecially when the merchant ſhips are generally deeper laden, and more weakly manned than the packets, and had from London the whole length of the river and channel to run before they left the land of England, while the packets had only to go from Falmouth, I could not but think the fact miſunderſtood or miſrepreſented. There happened then to be in London, a Nantucket ſea-captain of my acquaintance, to whom I communicated the affair. He told me he believed the fact might be true; but the difference was owing to this, that the Rhode-Iſland captains were acquainted with the gulf ſtream, which thoſe of the Engliſh packets were not. We are well acquainted with that ſtream, ſays he, becauſe in our purſuit of whales, which keep near the ſides of it, but are not to be met with in it, we run down along the ſides, and frequently croſs it to change our ſide: and in croſſing it have ſometimes met and ſpoke with thoſe packets, who were in the middle of it, and ſtemming it. We have informed them that they were

* Captain Truxton, on board whoſe ſhip this was written, has executed this propoſed machine; he has given ſix arms to the umbrella, they are joined to the ſtem by iron hinges, and the canvas is double. He has taken it with him to China. February 1786.

were ſtemming a current, that was againſt them to the value of three miles an hour; and adviſed them to croſs it and get out of it; but they were too wiſe to be counſelled by ſimple American fiſhermen. When the winds are but light, he added, they are carried back by the current more than they are forwarded by the wind: and if the wind be good, the ſubtraction of 70 miles a day from their courſe is of ſome importance. I then obſerved that it was a pity no notice was taken of this current upon the charts, and requeſted him to mark it out for me, which he readily complied with, adding directions for avoiding it in ſailing from Europe to North-America. I procured it to be engraved by order from the general poſt-office, on the old chart of the Atlantic, at Mount and Page's, Tower-hill; and copies were ſent down to Falmouth for the captains of the packets, who ſlighted it however; but it is ſince printed in France, of which edition I hereto annex a copy.

This ſtream is probably generated by the great accumulation of water on the eaſtern coaſt of America between the tropics, by the trade winds which conſtantly blow there. It is known that a large piece of water ten miles broad and generally only three feet deep, has by a ſtrong wind had its waters driven to one ſide and ſuſtained ſo as to become ſix feet deep, while the windward ſide was laid dry. This may give ſome idea of the quantity heaped up on the American coaſt, and the reaſon of its running down in a ſtrong current through the iſlands into the bay of Mexico, and from thence iſſuing through the gulph of Florida, and proceeding along the coaſt to the banks of Newfoundland, where it turns off towards and runs down through the Weſtern iſlands. Having ſince croſſed this ſtream ſeveral times in paſſing between America and Europe, I have been attentive to ſundry circumſtances relating to it, by which to know when one is in it; and beſides the gulph weed with which it is interſperſed, I find that

that it is always warmer than the ſea on each ſide of it, and that it does not ſparkle in the night: I annex hereto the obſervations made with the thermometer in two voyages, and poſſibly may add a third. It will appear from them, that the thermometer may be an uſeful inſtrument to a navigator, ſince currents coming from the northward into ſouthern ſeas, will probably be found colder than the water of thoſe ſeas, as the currents from ſouthern ſeas into northern are found warmer. And it is not to be wondered that ſo vaſt a body of deep warm water, ſeveral leagues wide, coming from between the tropics and iſſuing out of the gulph into the northern ſeas, ſhould retain its warmth longer than the twenty or thirty days required to its paſſing the banks of Newfoundland. The quantity is too great, and it is too deep to be ſuddenly cooled by paſſing under a cooler air. The air immediately over it, however, may receive ſo much warmth from it as to be rariſied and riſe, being rendered lighter than the air on each ſide of the ſtream; hence thoſe airs muſt flow in to ſupply the place of the riſing warm air, and meeting with each other, form thoſe tornados and water-ſpouts frequently met with, and ſeen near and over the ſtream; and as the vapour from a cup of tea in a warm room, and the breath of an animal in the ſame room, are hardly viſible, but become ſenſible immediately when out in the cold air, ſo the vapour from the gulph ſtream, in warm latitudes is ſcarcely viſible, but when it comes into the cool air from Newfoundland, it is condenſed into the fogs, for which thoſe parts are ſo remarkable.

The power of wind to raiſe water above its common level in the ſea, is known to us in America, by the high tides occaſioned in all our ſea-ports when a ſtrong northeaſter blows againſt the gulph ſtream.

The concluſion from theſe remarks is, that a veſſel from Europe to North-America may ſhorten her paſſage by avoiding to ſtem the ſtream, in which the thermometer will be

REMARKS

Upon the Navigation from

NEWFOUNDLAND to NEW-YORK,

In order to avoid the

GULPH STREAM

On one hand, and on the other the SHOALS *that lie to the Southward of Nantucket and of St. George's Banks.*

AFTER you have paſſed the Banks of Newfoundland to about the 44th degree of latitude, you will meet with nothing, till you draw near the Iſle of Sables, which we commonly paſs in latitude 43. Southward of this iſle, the current is found to extend itſelf as far North as 41° 20′ or 30′, then it turns towards the E. S. E. or S. E. ½ E.

Having paſſed the Iſle of Sables, ſhape your courſe for the St. George's Banks, ſo as to paſs them in about latitude 40°, becauſe the current ſouthward of thoſe banks reaches as far North as 39°. The ſhoals of thoſe banks lie in 41° 35′.

After having paſſed St. George's Banks, you muſt, to clear Nantucket, form your courſe ſo as to paſs between the latitudes 38° 30′ and 40° 45′.

The moſt ſouthern part of the ſhoals of Nantucket lie in about 40° 45′. The northern part of the current directly to the ſouth of Nantucket is felt in about latitude 38° 30′.

By obſerving theſe directions and keeping between the ſtream and the ſhoals, the paſſage from the Banks of Newfoundland to New-York, Delaware, or Virginia, may be conſiderably ſhortened; for ſo you will have the advantage of the eddy current, which moves contrary to the Gulph Stream. Whereas if to avoid the ſhoals you keep too far to the ſouthward, and get into that ſtream, you will be retarded by it at the rate of 60 or 70 miles a day.

The Nantucket whale-men being extremely well acquainted with the Gulph Stream, its courſe, ſtrength and extent, by their conſtant practice of whaling on the edges of it, from their iſland quite down to the Bahamas, this draft of that ſtream was obtained from one of them, Capt. Folger, and cauſed to be engraved on the old chart in London, for the benefit of navigators, by

B. FRANKLIN.

Note, The Nantucket captains who are acquainted with this ſtream, make their voyages from England to Boſton in as ſhort a time generally as others take in going from Boſton to England, viz. from 20 to 30 days.

A ſtranger may know when he is in the Gulph Stream, by the warmth of the water, which is much greater than that of the water on each ſide of it. If then he is bound to the weſtward, he ſhould croſs the ſtream to get out of it as ſoon as poſſible.

B. F.

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be very uſeful; and a veſſel from America to Europe may do the ſame by the ſame means of keeping in it. It may have often happened accidentally, that voyages have been ſhortened by theſe circumſtances. It is well to have the command of them.

But may there not be another cauſe, independent of winds and currents, why paſſages are generally ſhorter from America to Europe than from Europe to America? This queſtion I formerly conſidered in the following ſhort paper.

On board the Pennſylvania Packet, Capt. Oſborne,
At ſea, April 5, 1775.

"Suppoſe a ſhip to make a voyage eaſtward from a place in lat. 40° north, to a place in lat. 50° north, diſtance in longitude 75 degrees.

"In ſailing from 40 to 50, ſhe goes from a place where a degree of longitude is about eight miles greater than in the place ſhe is going to. A degree is equal to four minutes of time; conſequently the ſhip in the harbour ſhe leaves, partaking of the diurnal motion of the earth, moves two miles in a minute faſter, than when in the port ſhe is going to; which is 120 miles in an hour.

"This motion in a ſhip and cargo is of great force; and if ſhe could be lifted up ſuddenly from the harbour in which ſhe lay quiet, and ſet down inſtantly in the latitude of the port ſhe was bound to, though in a calm, that force contained in her would make her run a great way at a prodigious rate. This force muſt be loſt gradually in her voyage, by gradual impulſe againſt the water, and probably thence ſhorten the voyage. Query, In returning does the contrary happen, and is her voyage thereby retarded and lengthened?" *

Would it not be a more ſecure method of planking ſhips, if inſtead of thick ſingle planks laid horizontally, we were

* Since this paper was read at the Society, an ingenious member, Mr. Patterſon, has convinced the writer that the returning voyage would not, from this cauſe, be retarded.

to uſe planks of half the thickneſs, and lay them double and acroſs each other as in figure 23? To me it ſeems that the difference of expence would not be conſiderable, and that the ſhip would be both tighter and ſtronger.

The ſecuring of the ſhip is not the only neceſſary thing; ſecuring the health of the ſailors, a brave and valuable order of men, is likewiſe of great importance. With this view the methods ſo ſucceſsfully practiſed by Captain Cook in his long voyages, cannot be too cloſely ſtudied or carefully imitated. A full account of thoſe methods is found in Sir John Pringle's ſpeech, when the medal of the Royal Society was given to that illuſtrious navigator. I am glad to ſee in his laſt voyage that he found the means effectual which I had propoſed for preſerving flour, bread, &c. from moiſture and damage. They were found dry and good after being at ſea four years. The method is deſcribed in my printed works, page 452, fifth edition. In the ſame, page 469, 470, is propoſed a means of allaying thirſt in caſe of want of freſh water. This has ſince been practiſed in two inſtances with ſucceſs. Happy if their hunger, when the other proviſions are conſumed, could be relieved as commodiouſly; and perhaps in time this may be found not impoſſible. An addition might be made to their preſent vegetable proviſion, by drying various roots in ſlices by the means of an oven. The ſweet potatoe of America and Spain, is excellent for this purpoſe. Other potatoes, with carrots, parſnips and turnips, might be prepared and preſerved in the ſame manner.

With regard to make-ſhifts in caſes of neceſſity, ſeamen are generally very ingenious themſelves. They will excuſe however the mention of two or three. If they happen in any circumſtance, ſuch as after ſhipwreck, taking to their boat, or the like, to want a compaſs, a fine ſewing-needle laid on clear water in a cup will generally point to the north, moſt of them being a little magnetical, or may be made ſo by being ſtrongly rubbed or hammered,

ed, lying in a north and ſouth direction. If their needle is too heavy to float by itſelf, it may be ſupported by little pieces of cork or wood. A man who can ſwim, may be aided in a long traverſe by his handkerchief formed into a kite, by two croſs ſticks extending to the four corners; which being raiſed in the air, when the wind is fair and freſh, will tow him along while lying on his back. Where force is wanted to move a heavy body, and there are but few hands and no machines, a long and ſtrong rope may make a powerful inſtrument. Suppoſe a boat is to be drawn up on a beach, that ſhe may be out of the ſurf, a ſtake drove into the beach where you would have the boat drawn; and another to faſten the end of the rope to, which comes from the boat, and then applying what force you have to pull upon the middle of the rope at right angles with it, the power will be augmented in proportion to the length of rope between the poſts. The rope being faſtened to the ſtake A, and drawn upon in the direction C D, will ſlide over the ſtake B; and when the rope is bent to the angle A D B, repreſented by the pricked line in figure 24, the boat will be at B.

Some ſailors may think the writer has given himſelf unneceſſary trouble in pretending to adviſe them; for they have a little repugnance to the advice of landmen, whom they eſteem ignorant and incapable of giving any worth notice; though it is certain that moſt of their inſtruments were the invention of landmen. At leaſt the firſt veſſel ever made to go on the water was certainly ſuch. I will therefore add only a few words more, and they ſhall be addreſſed to paſſengers.

When you intend a long voyage, you may do well to keep your intention as much as poſſible a ſecret, or at leaſt the time of your departure; otherwiſe you will be continually interrupted in your preparations by the viſits of friends and acquaintance, who will not only rob you of the time you want, but put things out of your mind, ſo

that when you come to ſea, you have the mortification to recollect points of buſineſs that ought to have been done, accounts you had intended to ſettle, and conveniencies you had propoſed to bring with you, &c. &c. all which have been omitted through the effect of theſe officious friendly viſits. Would it not be well if this cuſtom could be changed; if the voyager after having, without interruption, made all his preparations, ſhould uſe ſome of the time he has left, in going himſelf to take leave of his friends at their own houſes, and let them come to congratulate him on his happy return.

It is not always in your power to make a choice in your captain, though much of your comfort in the paſſage may depend on his perſonal character, as you muſt for ſo long a time be confined to his company, and under his direction; if he be a ſenſible, ſociable, good natured, obliging man, you will be ſo much the happier. Such there are; but if he happens to be otherwiſe, and is only ſkilful, careful, watchful and active in the conduct of his ſhip, excuſe the reſt, for theſe are the eſſentials.

Whatever right you may have by agreement in the maſs of ſtores laid in by him for the paſſengers, it is good to have ſome particular things in your own poſſeſſion, ſo as to be always at your own command.

1. Good water, that of the ſhip being often bad. You can be ſure of having it good only by bottling it from a clear ſpring or well and in clean bottles. 2. Good tea. 3. Coffee ground. 4. Chocolate. 5. Wine of the ſort you particularly like, and cyder. 6. Raiſins. 7. Almonds 8. Sugar. 9. Capillaire. 10. Lemons. 11. Jamaica ſpirits. 12. Eggs greas'd. 13. Diet bread. 14. Portable ſoup. 15. Ruſks. As to fowls, it is not worth while to have any called yours, unleſs you could have the feeding and managing of them according to your own judgment under your own eye. As they are generally treated at preſent in ſhips, they are for the moſt part ſick, and their fleſh tough

tough and hard as whitleather. All ſeamen have an opinion, broached I ſuppoſed at firſt prudently, for ſaving of water when ſhort, that fowls do not know when they have drank enough, and will kill themſelves if you give them too much, ſo they are ſerved with a little only once in two days. This is poured into troughs that lie ſloping, and therefore immediately runs down to the lower end. There the fowls ride upon one another's backs to get at it, and ſome are not happy enough to reach and once dip their bills in it. Thus tantalized, and tormented with thirſt, they cannot digeſt their dry food, they fret, pine, ſicken and die. Some are found dead, and thrown overboard every morning, and thoſe killed for the table are not eatable. Their troughs ſhould be in little diviſions like cups to hold the water ſeparately, figure 25. But this is never done. The ſheep and hogs are therefore your beſt dependance for freſh meat at ſea, the mutton being generally tolerable and the pork excellent.

It is poſſible your captain may have provided ſo well in the general ſtores, as to render ſome of the particulars above recommended of little or no uſe to you. But there are frequently in the ſhip poorer paſſengers, who are taken at a lower price, lodge in the ſteerage, and have no claim to any of the cabbin proviſions, or to any but thoſe kinds that are allowed the ſailors. Theſe people are ſometimes dejected, ſometimes ſick, there may be women and children among them. In a ſituation where there is no going to market, to purchaſe ſuch neceſſaries, a few of theſe your ſuperfluities diſtributed occaſionally may be of great ſervice, reſtore health, ſave life, make the miſerable happy, and thereby afford you infinite pleaſure.

The worſt thing in ordinary merchant ſhips is the cookery. They have no profeſſed cook, and the worſt hand as a ſeaman is appointed to that office, in which he is not only very ignorant but very dirty. The ſailors have

have therefore a ſaying, that *God ſends meat and the devil cooks.* Paſſengers more piouſly diſpoſed, and willing to believe heaven orders all things for the beſt, may ſuppoſe that knowing the ſea-air and conſtant exerciſe by the motion of the veſſel would give us extraordinary appetites, bad cooks were kindly ſent to prevent our eating too much; or, that foreſeeing we ſhould have bad cooks, good appetites were furniſhed to prevent our ſtarving. If you cannot truſt to theſe circumſtances, a ſpirit-lamp, with a blaze-pan, may enable you to cook ſome little things for yourſelf; ſuch as a haſh, a ſoup, &c. And it might be well alſo to have among your ſtores ſome potted meats, which if well put up will keep long good. A ſmall tin-oven to place with the open ſide before the fire, may be another good utenſil, in which your own ſervant may roaſt for you a bit of pork or mutton. You will ſometimes be induced to eat of the ſhip's ſalt beef, as it is often good. You will find cyder the beſt quencher of that thirſt which ſalt meat or fiſh occaſions. The ſhip biſcuit is too hard for ſome ſets of teeth. It may be ſoftened by toaſting. But ruſk is better; for being made of good fermented bread, ſliced and baked a ſecond time, the pieces imbibe the water eaſily, ſoften immediately, digeſt more kindly and are therefore more wholſome than the unfermented biſcuit. By the way, ruſk is the true original biſcuit, ſo prepared to keep for ſea, biſcuit in French ſignifying twice baked. If your dry peas boil hard, a two-pound iron ſhot put with them into the pot, will by the motion of the ſhip grind them as fine as muſtard.

The accidents I have ſeen at ſea with large diſhes of ſoup upon a table, from the motion of the ſhip, have made me wiſh that our potters or pewterers would make ſoup-diſhes in diviſions, like a ſet of ſmall bowls united togegether, each containing about ſufficient for one perſon, in ſome ſuch form as fig. 26; for then when the ſhip ſhould make a ſudden heel, the ſoup would not in a body flow over

over one ſide, and fall into people's laps and ſcald them, as is ſometimes the caſe, but would be retained in the ſeparate diviſions, as in figure 27.

After theſe trifles, permit the addition of a few general reflections. Navigation when employed in ſupplying neceſſary proviſions to a country in want, and thereby preventing famines, which were more frequent and deſtructive before the invention of that art, is undoubtedly a bleſſing to mankind. When employed merely in tranſporting ſuperfluities, it is a queſtion whether the advantage of the employment it affords is equal to the miſchief of hazarding ſo many lives on the ocean. But when employed in pillaging merchants and tranſporting ſlaves, it is clearly the means of augmenting the maſs of human miſery. It is amazing to think of the ſhips and lives riſqued in fetching tea from China, coffee from Arabia, ſugar and tobacco from America, all which our anceſtors did well without. Sugar employs near one thouſand ſhips, tobacco almoſt as many. For the utility of tobacco there is little to be ſaid; and for that of ſugar, how much more commendable would it be if we could give up the few minutes gratification afforded once or twice a day by the taſte of ſugar in our tea, rather than encourage the cruelties exerciſed in producing it. An eminent French moraliſt ſays, that when he conſiders the wars we excite in Africa to obtain ſlaves, the numbers neceſſarily ſlain in thoſe wars, the many priſoners who periſh at ſea by ſickneſs, bad proviſions, foul air, &c. &c. in the tranſportation, and how many afterwards die from the hardſhips of ſlavery, he cannot look on a piece of ſugar without conceiving it ſtained with ſpots of human blood! Had he added the conſideration of the wars we make to take and retake the ſugar iſlands from one another, and the fleets and armies that periſh in thoſe expeditions, he might have ſeen his ſugar not merely ſpotted, but thoroughly dyed ſcarlet in grain. It is theſe wars that make the maritime powers of Europe, the

the inhabitants of London and Paris, pay dearer for ſugar than thoſe of Vienna, a thouſand miles from the ſea; becauſe their ſugar coſts not only the price they pay for it by the pound, but all they pay in taxes to maintain the fleets and armies that fight for it.

With great eſteem, I am, Sir,

Your moſt obedient humble ſervant,

B. FRANKLIN.

Obſervations

Obſervations of the warmth of the ſea-water, &c. by Fahrenheit's thermometer, in croſſing the Gulph ſtream; with other remarks made on board the Pennſylvania packet, Capt. Oſborne, bound from London to Philadelphia, in April and May 1775.

Date.	Hour.	Temp. of Air.	Temp. of Wat.	Wind.	Courſe.	Diſtance.	Latitude N.	Longitude W.	Remarks.
April 10			62						
11			61						
12			64						
13			65						
14			65				o /	o /	
26		60	70				37 39	60 38	Much gulph weed; ſaw a whale.
27		60	70	S S E	W b S		37 13	62 29	Colour of water changed.
28	8A.M.	70	64	S W	W N W		37 48	64 35	No gulph weed.
—	6 P.M.	67	60			34			Sounded, no bottom.
29	8A.M.	63	71	N	W	44	37 26	66 o	Much light in the water laſt nig.
—	5 P.M.	65	72	N E					Water again of the uſual deep ſea
—	11 dit.	66	66	N W b N	W b S	57			colour, little or no light in it at
30	8A.M.	64	70	N E	W b N	69			night.
—	12	62	70		E b S	24	37 20	68 53	Frequent gulph weed, water con-
—	6 P.M.	64	72	E S E	W b N	43			tinues of ſea colour, little light.
—	10 dit.	65	65	S		25			Much light.
May 1	7A.M.	68	63			60			Much light all laſt night.
—	12	65	56	S S W	W N W	44	38 13	72 23	Colour of water changed.
—	4 P.M.	64	56		W b N	21			
—	10 dit.	64	57	S W	W N W	31			Much light.
2	8A.M.	62	53			18	38 43	74 3	Much light. Thunder-guſt.
—	12	60	53	W S W	N W	18			
—	6 P.M.	64	55	N W	W S W	15			
—	10 dit.	65	55	N b W	W b N	10			
3	7 A.M.	62	54			30	38 30	75 o	

Obſervations of the warmth of the ſea-water, &c. by Fahrenheit's thermometer; with other remarks made on board the Repriſal, Capt. Wycks, bound from Philadelphia to France, in October and November 1776.

Date.	Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Courſe.	Diſtance.	Latit. N.	Long. W.	Remarks.
Octo. 31	10		76	70	SSE	E b S	135	38 12	70 30	Left the capes Thurſday night, October 29, 1776.
		4		71						
Nov. 1	10			78	W S W	E ½ N	109	No ob.	68 12	
		4	71	81						
2	8		71	75	N					
	12			78			141	ditto.	65 23	
		4	67	76						Some ſparks in the water theſe two laſt nights.
3	8			76	N W	E S E ½ E				
	12			76		E b S	160	37 0	62 7	
		4	70	76						Ditto.
4	9		68	76		N b E				
		1		76			194	36 26	58 8	
		4	68	76						
		8		78						Ditto.
5	8		68	76		N E				
	12		70	75			163	35 21	55 3	
		4		75						
		8		75						
6	8			76	E b N	S 50 E				
	12			77			75	35 33	53 52	
7	8			78	S E b E	N 30 W				
	12			77			108	36 6	52 46	
		4		77						
8	9		75	77	S b E	N 49 E				
	12			77			175	38 2	50 1	
		4		77						
9	9		75	77						
	12		75	70	S W	N 33 E	175	39 39	46 55	

Obſervations made on board the Repriſal, continued.

Date.	Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Courſe.	Diſtance.	Latit. N.	Long. W.	Remarks.
Novem. 9		4		71						
10	8		70	68						
—	12			64	E	N 17 E	64	40 39	46 27	
11	8			63						
—	12			61	S E	N 8 E	41	41 19	46 19	
12	8		56	59						
—		4		69	N N W	N 80 E	120	41 39	43 42	
13	all day			68	E	S 82 E	69	41 29	42 10	
14	8		70	70		N 74 E	111	42 0	39 57	
—		Noon		72	E S E					
—		4		71						
15	8		61	69						
—		Noon		68	W S W	N 70 E	186	43 3	35 51	
—		4		67						
16		Noon	65	67	S W	N 67 W	48	43 22	34 50	
—		4		63						
17	8			63	E S E	N 19 E	56	44 15	34 25	
18	all day			65	S b W	N 75 E	210	45 6	29 43	Some gulph weed.
19		Noon	65	64	S W	N 80 E	238	45 46	24 2	
20	8			62	N	S 80 E	155	45 19	20 30	
—		4		60						
21	9			62	S	N 88 E	94	45 22	18 17	
22	10		60	62	S S W	S 89 E	133	45 19	15 19	
23		Noon		61	W S W	S 86 E	194	45 6	10 35	
24		do.		60	N N E	N 78 E	191	45 46	6 10	
25		do.		60	N E	S 76 E	125	45 4	3 23	
26		do.	56	60	E	N 73 E	31	45 13	2 20	
27		do.		58						Soundings off Belliſle.
28		do.	54	56						

Dates.	Latit. N.	Long. W.	Therm. A.M. Air.	Therm. A.M. Water.	Therm. P. M. Air.	Therm. P. M. Water.	Winds.	Course.	Distance.	Variation of the Needle.	Therm. Noon. A.	Therm. Noon. W.
July 29			62	57			{ These are taken on an average of 24 hours. }					
30			62	58	63	58						
31			60	58	62	62			Miles.	West.		
August 1	49 15	4 15	63	62	60	64	East	S W ½ W	60	22° 0		
2	48 28	8 58	64	64	64	63	E S E	W b S ½ S	174			
3	47 0	12 13	60	67	omitted.		N E	S W b W	160			
4	45 0	15 43	66	66	do.	66	N W b W	S W ½ W	190			
5	43 5	17 25	67	65	65	68	N E	S W b S	131	20 0		
6	41 3	19 44	70	68	71	69	N E	S W ½ S	166	16 30		
7	38 45	21 34	70	70	68	70	N E	S S W ¾ W	165	11 30		
8	36 42	23 10	72	71	73	72	N E	S S W ½ W	149	11 15		
9	35 40	25 40	73	73	73	74	N E	W S W ¼ S	137			
10	35 0	27 0	71	73	77	75	N W	W S W ½ S	76			
11	33 51	28 42	74	74	76	77	North	S W ¾ W	112			
12	33 30	31 30	76	75	76	76	North	W ¾ S	143		77	78
13	33 17	33 32	76	76	78	77	N E	W ½ S	103		81	79
14	33 22	34 31	76	76	81	79	S S E	W ½ N	50		79	79
15	33 45	35 0	78	79	79	78	W N W	S W ¼ W	35		81	80
16	34 14	35 30	79	78	81	80	West	N W ½ N	38		80	78
17	35 37	36 4	80	79	80	78	W S W	N N W	75		80	79
18	36 7	37 16	80	78	omitted.		N W b W	W N W ½ N	65		79	77
19	36 38	38 0	78	77	78	77	W S W	N W ½ W	49		77	75
20	37 38	38 6	78	76	omitted.		West	N ¼ W	62		77	75
21	36 15	38 26	73	74	78	76	W N W	S b W	82		80	77
22	35 40	38 44	77	76	80	77	W b S	S S W	38		omitted.	
23	35 35	40 52	79	77	78	75	North	W ¼ S	100		75	74
24	35 12	41 31	75	73	75	74	W N W	S W b W	41		80	76
25	35 40	42 33	79	76	79	76	W b N	W N W ¾ N	60		80	76
26	35 30	42 44	79	76	80	76	S W b W	S W ½ S	14		81	78
27	35 14	43 23	79	77	81	79	West	W S W ¼ S	38		78	78
28	34 23	44 0	78	76	78	78	N N E	S W b S	60		79	78
29	34 12	45 52	77	78	78	78	N E	W ¼ S	94	8 0	78	78
30	34 5	48 31	78	78	78	78	East	W ½ S	134		80	80
31	34 20	51 4	80	79	81	79	East	W ¾ S	129		83	80
September 1	34 20	52 47	81	78	omitted.		S S W	W ¼ N	86		83	80
2	34 55	55 12	81	80	83	80	S W	W b N ½ W	125		84	81
3	35 30	57 24	83	80	83	80	S W b S	W b N ½ N	114	6 0	83	81
4	35 50	59 1	82	80	83	80	S W ½ W	W b N ¼ N	82		82	81
5	35 55	61 0	81	80	82	81	S S W	W ¼ N	96		78	80
6	36 20	62 30	80	81	79	80	N W b N	W b N	75		78	81
7	34 50	63 10	87	80	78	81	N W b W	S, S W	86		75	79
8	34 45	64 40	75	79	75	79	North	W ¼ S	74		78	80
9	35 43	66 42	75	79	77	73	N E	W N W	108		78	72
10	37 20	68 40	77	73	77	70	E N E	N W	126			

N. B. Longitude is reckoned from London, and the Thermometer is according to Fahrenheit.

OBSERVATIONS.

July 31. At one P. M. the Start bore W N W. diſtant ſix leagues.

Auguſt 1. The water appears luminous in the ſhip's wake.

—2. The temperature of the water is taken at eight in the morning and at eight in the evening.

—6. The water appears leſs luminous.

—7. Formegas S W. diſt. 32½ deg. St. Mary's S W ½ S 33 leagues.

—8. From this date the temperature of the water is taken at eight in the morning and at ſix in the evening.

—10. Moonlight, which prevents the luminous appearance of the water.

—11. A ſtrong ſoutherly current.

—12. Ditto. From this date the temperature of the air and water was taken at noon, as well as morning and evening.

—16. Northerly current.

—19. Firſt ſaw gulph weed.

—21. Southerly current.

—22. Again ſaw gulph weed.

—24. The water appeared luminous in a ſmall degree before the moon roſe.

—29. No moon, yet very little light in the water.

—30. Much gulph weed to-day.

—31. Ditto.

Sept. 1. Ditto.

—2. A little more light in the water.

—4. No gulph weed to-day. More light in the water.

—5. Some gulph weed again.

—6. Little light in the water. A very hard thunder-guſt in the night.

—7. Little gulph weed.

—8. More light in the water. Little gulph weed.

—9. Little gulph weed. Little light in the water laſt evening.

—10. Saw ſome beds of rock-weed; and we were ſurpriſed to obſerve the water ſix degrees colder by the thermometer than the preceding noon.

This day (10th) the thermometer ſtill kept deſcending, and at five in the morning of the 11th, it was in water as low as 70, when we ſtruck ſoundings. The ſame evening the pilot came on board, and we found our ſhip about five degrees of longitude a-head of the reckoning, which our captain accounted for by ſuppoſing our courſe to have been near the edge of the gulph ſtream, and thus an eddy-current always in our favour. By the diſtance we ran from Sept. 9, in the evening, till we ſtruck ſoundings, we muſt have then been at the weſtern edge of the gulph ſtream, and the change in the temperature of the water was probably owing to our ſuddenly paſſing from that current, into the waters of our own climate.

On the 14th of Auguſt the following experiment was made. The weather being perfectly calm, an empty bottle, corked very tight, was ſent down 20 fathoms, and it was drawn up ſtill empty. It was then ſent down again 35 fathoms, when the weight of the water having forced in the cork, it was drawn up full; the water it contained was immediately tried by the thermometer, and found to be 70, which was ſix degrees colder than at the ſurface: The lead and bottle were viſible, but not very diſtinctly ſo, at the depth of 12 fathoms but when only 7 fathoms deep, they were perfectly ſeen from the ſhip. This experiment was thus repeated Sept. 11, when we were in ſoundings of 18 fathoms. A keg was previouſly prepared with a valve at each end, one opening inward the other outward; this was ſent to the bottom in expectation that by the valves being both open when going down, and both ſhut when coming up, it would keep within it the water received at bottom. The upper valve performed its office well, but the under one did not ſhut quite cloſe, ſo that much of the water was loſt in hauling it up the ſhip's ſide. As the water in the keg's paſſage upwards could not enter at the top, it was concluded that what water remained in it was of that near the ground, and on trying this by the thermometer, it was found to be at 58, which was 12 degrees colder than at the ſurface.

This laſt Journal was obligingly kept for me by Mr. J. Williams, my fellow-paſſenger in the London Packet, who made all the experiments with great exactneſs.

Two

N° XXXIX.

Two Hearts found in one Partridge.

ALL the works of nature are linked the one to the other and form a whole, in the immenſity of which we only perceive ſome points which appear to us detached, becauſe thoſe which unite them are concealed from us.

The reſult of this connection is, that no work of nature ought to be neglected, that there is not one which may not derive ſome direct or indirect utility to man.

That which appears futile, ſhould be graſped like the others, and in poſſeſſing ourſelves of it we ſhould be aſſured that we have hold of a chain, the precious links of which will be diſcovered by time[a]. If thoſe links which are wanting leave vacancies, the intermediate links are every day preſented to us by the hand of chance; and it is the buſineſs of the naturaliſt to arrange them. Let us then offer

DEUX COEURS TROUVÉS DANS UNE PERDRIX.

TOUS les faits de la nature ſont liés les uns aux autres, et forment un tout, dans l'immenſité duquel nous n'appercevons que quelques points, qui nous paroiſſent iſolés parce-que ceux qui les uniſſent nous ſont cachés. Il reſulte de cet enchainement qu'aucun fait de la nature ne doit être négligé, qu'il en eſt pas un qui ne puiſſe devenir de quelque utilité pour l'homme, ſoit directe ou indirecte. Celui qui paroit le plus futile doit être recueilli comme les autres; en le faiſiſſant on doit être aſſuré qu'on tient une chaine ou le temps découvrira des chainons precieux*. Si ceux qui manquent y laiſſent des lacunes, ces chainons intermediaires nous ſont tous les jours preſentés par la main du hazard, et celle du naturaliſte les met à leurs places. Offrons

(a) We here ſee women ſit quiet in their houſes whilſt thunder is rumbling over their heads; would they, at this day, enjoy this happy ſecurity, if a man had not obſerved, ſome thouſand years ago, that a piece of amber when rubbed attracted light bodies which are near it. It is he who put into the hands of modern philoſophers the chain in which Franklin was to find the link, from which his imagination took the hint of his conductor.

* *Nous voyons ici les femmes tranquilles dans leurs maiſons lorſque la tonnérre gronde ſur leur tête. Jouiroient elles aujourdhui de cette heureuſe ſécurité, ſi un homme n'avoit obſervé il y a quelques milliers d'années, qu'un morceau d'ambre frotté attire les corps legers qui l'avoiſinent? C'eſt lui qui a mis dans les mains des phyſiciens modernes la chaine ou Franklin devoit trouver le chainon dont il eſt parti pour imaginer ſon conducteur.*

fer him all thoſe which we meet with. It increaſes the maſs of human knowledge, and enriches a ſtore-houſe which is very precious to man; a ware-houſe belonging to all nations and to all ages. Not to lodge every new diſcovery in this common ſtore, is to ſquander away riches which we held only in truſt, and in which the moſt diſtant generations have an intereſt. This is criminally depriving humanity of a bleſſing which is intended for the good of ſociety.

The foregoing reflections induce me to publiſh the following fact. Monſieur Vergé, ſenior ſurgeon of the artillery, lodged next door to me, and came into my room with the entrails of a partridge which he had juſt opened, and ſhewed me two hearts attached to one lung by blood veſſels. I requeſted him to go immediately to the chev. de Chaſtellux, knowing that the phenomenon would be intereſting to him. But wiſhing firſt to ſtep home, and not being able with one hand to open the padlock which faſtened his door, he put down the ſaucer which contained the entrails; his dog who had followed, ſuppoſing it was a meſs for himſelf, ſoon emptied the ſaucer.

The chevalier de Chaſtellux called on me an hour afterwards to ſee a male opoſſum, of which I ſpoke to him on account

Offrons lui donc tous ceux que nous rencontrons, c'eſt groſſir la maſſe des connoiſſances humaines, et enrichir le magazin le plus précieux à l'homme, magazin qui appartient à toutes les nations et à tous les ſiecles. Ne pas rapporter à ce depôt commun les faits nouveaux que l'on découvre, c'eſt perdre des richeſſes dont on étoit dépoſitaire, et ſur leſquelles les générations les plus reculées avoient des droits. C'eſt ſe rendre coupable envers l'humanité, en la fruſtrant d'un bien dont on lui étoit comptable.

Ce ſont ces réflexions qui m'engagent à rendre publique le fait ſuivant.

Le Sieur Vergé, chirurgien major de l'artillerie, logé à coté de chèz moi, entra il y a quelques jours dans ma chambre, tenant dans ſes mains les entrailles d'une perdrix qu'il venoit d'ouvrir, et m'y fit remarquer deux coeurs qui tenoient à un même poumon par des vaiſſeaux ſanguins. Je le priai d'aller ſur le champ les faire voir à Mr. le Chevr. de Chaſtellux, que je ſavois que ce phénomene intereſſeroit. Mais ayant voulu auparavant rentrer chez lui, et ne pouvant d'une main ouvrir le cadenat qui ferme ſa porte, il poſa a terre la ſoucoupe ſur la quelle etoient ces entrailles. Son chien, qui l'avoit ſuivi, crut que c'etoit pour lui, et eut bientôt vidé la ſoucoupe. Mr. le Chevr. de Chaſtellux vint une heure aprés chez moi, voir un opoſſum mâle, dont je lui avois parlé à cauſe de la ſingularité des organes de la génération de

ces

account of the ſingularity of his organs of generation[b]. I mentioned to him the two hearts, he could not comprehend me. I ſent to aſk of the ſenior ſurgeon why he did not do what I requeſted of him; he came and related the accident of the ſaucer. The chevalier de Chaſtellux judged there was no other way of repairing the loſs but by a certificate, ſtating what we had ſeen; but every thing conſpired againſt the deſire the chevalier de Chaſtellux and I had to perpetuate the phenomenon; the dog had deprived us of the monument; the maſter refuſed us his certificate, ſaying, one cannot be too circumſpect in affirming a fact which we ought not to judge of from external appearances; but to be convinced that that which appeared to us as two hearts were really ſo, they ought to have been diſſected, &c. One cannot too much applaud the ſcruples of Mr. Vergé; but regarding as much my veracity as Mr. Vergé his, I am not afraid to hazard an exact relation of what I have ſeen, without alteration or exaggeration. I ſaw, then, two fleſhy ſubſtances of a brown violet colour, of an oblong form, thicker at one end than the other, nearly round taken tranſverſely. Theſe two ſubſtances reſembled each

cet animal†. Je lui parlai des deux coeurs; il ne ſcut ce que je lui voulois dire: J'énvoyai demander au chirurgèon major pourquoi il n'avoit pas exécuté ce que je lui avoit dit; il vint nous raconter l'accident de la ſoucoupe. Mr. le Chevr. de Chaſtellux jugea qu'l n'y avoit pas d'autre moyen de réparer cette perte, que par un proces verbal, qui conſtata ce que nous avions vu. Mais tout conſpiroit contre le deſir que Mr. le Chevr. de Chaſtellux et moi avions de perpétuer le ſouvenir de ce phenomene: Le chien nous a privé du monument; le maitre nous refuſe ſon atteſtation, diſant, *qu'on ne peut être trop circonſpect à affirmer un fait; qu'il ne faut pas juger ſur des apparences exterieurs; que pour etre aſſuré que ce qui nous a paru des coeurs fuſſent reellement des coeurs, qu'il eut fallu les avoir ouverts, &c.* On ne peut qu'aplaudir aux ſentiments qui rendent M. Vergé ſi ſcrupuleux. Mais en me piquant d'autant de véracité que M. Vergé, je ne crains point de la compromettre en rendant exactement ce que j'ai vu, ſans altération et ſans exagération. Or j'ai vû deux corps charnus d'un violet brun, de forme un peu oblongue, plus gros par un bout que par l'autre, un peu plus convexe d'un coté que de l'autre, a peu près ronds conſidérés tranſverſalement. Ces deux corps etoient abſolument ſemblables l'un a l'autre

(b) It has nothing external except the two teſticles placed under the belly; the male and the female have but one orifice placed under the tail, which ſerves them to void their excrements, their urine, and probably for generation. It is not known whether a penis comes out of this orifice, or whether there is nothing but an opening of the two orifices in the act of copulation.

† *Il n'a d'exterieur que deux teſticules placées ſous le ventre, le male et la femelle n'ont qu'un orifice placé ſous la quéue qui leur ſert a vider les excréments, les urines, et probablement à la génération. On ignore ſi par ce cloaque le mâle ſort une verge, ou s'il ne ſe fait qu'un abouchement des deux orifices lors de la copulation.*

each other exactly in ſize, form and colour. From their thicker ends came out ſeveral veſſels, among them I obſerved one to each of the bodies pretty large and paler than the reſt; a part of this veſſel was faſtened to the lungs in ſuch a manner, that by taking hold of only the lungs, theſe two bodies in queſtion were both ſuſpended at the ſame diſtance from the lungs. In placing theſe viſcera on the hand in ſuch a manner that the reſembling ſides of theſe two bodies faced each other, the veſſels which were ſuſpended appeared uniformly diſpoſed, which makes me imagine that the two bodies were uniformly diſpoſed in the body of the animal[c]. However, the above obſervations being freſh in my memory, I ordered the entrails of four dozen of partridges of the ſame ſpecies[d], juſt taken out, to be brought me; and I found in each a body like the double body which had been found in the bird of Mr. Vergé, having ſimilar veſſels, ſome of which were paler than others, adhering in the ſame manner to the lungs, and this body was certainly a heart or my partridges had none. I nevertheleſs leave every one at liberty to judge

l'autre, en volume, en forme, et en couleur; de leur gros bout ſortoient pluſieurs vaiſſeaux, parmi leſquels j'en remarquai un à chacun de ces corps, aſsèz gros et de couleur plus pale que les autres; une partie de ces vaiſſeaux tenoient au poumon, de maniére que faiſiſſant ſeulement le poumon, les deux corps dont il s'agit y reſtoient ſuſpendus tous deux a la même diſtance du poumon. En arrangeant ces viſceres ſur la main, de maniére que les cotes ſemblables de ces deux corps ſe regardaſſent, les vaiſſeaux qui y tenoient ſe trouvoient ſimetriquement diſpoſés; ce qui me fait juger que ces deux corps devoient ſe trouver dans l'animal ſimetriquement placés*. Enfin ayant encore la memoire fraiche des obſervations ſuſdites, je me ſis apporter les entrailles de quatre douzaines de perdrix de la même eſpece† que l'on venoit de vider, et j'ai vû qu'il ſe trouvoit dans chacune un corps en tout ſemblable à celui qui s'etoit trouvé double dans la perdrix de M. Vergé, ayant de pareils vaiſſeaux, dont un plus pâle que les autres, tenant de la même maniére au poumon, et ce corps etoit certainement un cœur, ou mes perdrix n'en avoient pas. Je laiſſe maintenant chacun libre de juger lequel paroit le plus vraiſemblable, qu'une perdrix eut deux cœurs, ou qu'il ne s'en trouva pas un ſeul dans quarante

* *Je preſume qu'ils occupoient les deux cotés du thorax, et que les veines de l'un s'anaſtomoſoient aux artéres de l'autre et reciproquement.*

(c) I preſume that they occupied the two ſides of the thorax, and that the veins of the one anaſtamoſed with the arteries of the other, and vice verſa.

† *Elle eſt un peu plus petite que celle qu'on appelle en France perdrix de paſſage ou raquette, elle ſe perche de meme.*

(d) It appears rather ſmaller than that which, in France, is called perdrix de paſſage, or raquette; they ſit on a tree in the ſame manner.

which was the moſt probable, that one partridge had two hearts, or that forty-eight had none at all. It may indeed be objected, that the firſt might have a true and a falſe one. Mr. Voltaire, who believes that nature amuſes herſelf in making concha veneris, might alſo believe that ſhe diverted herſelf in imitating two hearts. I ſhould therefore be as circumſpect as Mr. Vergé, and not affirm ſooner than him that his partridge had two hearts, for I may be miſtaken. I am, however, infinitely more certain of it than that it had but one.

D'ABOVILLE.

Williamſburgh, Feb. 15, 1782.

I the ſubſcriber, ſenior ſurgeon of the regiment of Auxonne, and of the artillerymen of the king's army in America, do declare, that the 10th of this month, having taken out the entrails of the body of a partridge, there appeared to me two hearts. However, during the ſhort time that I had to inſpect them, I was ſo aſtoniſhed with ſo ſurprizing a phenomenon, that I fear my eyes may have deceived me, and I dare not to affirm what I believe I ſaw. I went immediately to a merchant, who lodged next door to me, to ſhow him this miracle, and alſo to Mr. Aboville. The latter has juſt read to me the account he has written of this phenomenon as it appeared to him. I hereby certify that I ſaw

quarente huit perdrix. Il reſtera à dire que la premiére pouvoit en avoir un vrai et un faux. M. de Voltaire, qui croyoit que la nature s'amuſe a imiter des conqua véneris, auroit pu croire qu'elle s'amuſe auſſi a contrefaire des cœurs; je ſerai donc auſſi circonſpect que M. Vergé, et n'affirmerai pas plus que lui que ſa perdrix avoit deux cœurs, car je pourrois me tromper; j'en ſuis cependant infiniment plus certain que je ne le ſuis de n'en avoir qu'un.

A Williamſburg en Virginie, le 15 Fev. 1782.

(Signé) d'Aboville.

JE ſouſſigné, chirurgien major du régiment d'Auxonne, et de l'equipage d'artillerie de l'armée du roi en Amerique; déclare, que le dix de ce mois ayant rétiré les entrailles du corps d'une perdrix, il me parut qu'il s'y trouvoit deux coeurs. Mais pendant le peu de temps que j'ai pu les conſidérer, j'étois tellement ebloui d'un phénomene auſſi ſurprenant, que je crains que mes yeux ne m'ayent trompé, et n'oſe affirmer ce que je crois avoir vû. Je fus ſur le champ montrer cette merveille a un marchand qui loge a coté de chez moi, et à M. d'Aboville; ce dernier vient de me lire l'expoſé qu'il a écrit de ce phénomene, tel qu'il s'eſt preſenté

I ſaw nothing contrary to the obſervations contained in this account.

Williamſburgh, Feb. 15, 1782. VERGÉ.

I the ſubſcriber, merchant at Williamſburgh, reſiding next door to Mr. Vergé, ſenior ſurgeon of artillery, do certify that the 10th of this month, this ſurgeon came to ſhew me the entrails of a partridge, and pointed out to me two hearts very diſtinct, adhering by their blood-veſſels to the ſame lung. I moreover certify that Mr. Aboville has read me the account he has given thereof, and that I find nothing in it contrary to what I ſaw.

THOMAS BENTLEY.

Williamſburgh, Feb. 18, 1782.

ſenté a ſes yeux. Je certifie n'avoir rien obſervé qui ne ſoit conforme aux obſervations contenues dans cet expoſé.

A Williamſburg, le 18 Fev. 1782.
(Signé) Vergé.

Je ſouſſigné marchand demeurant à Williamſburg à coté de chez M. Vergé, chirurgien major de l'artillerie, certifie, que le dix de ce mois ce chirurgien vint me montrer les entrailles d'une perdrix, et qu'il m'y fit remarquer deux cœurs, bien diſtincts, tenans tous les deux par leurs vaiſſeaux ſanguins, à un méme poumon. Je certifie en outre que M. d'Aboville vient de me lire l'expoſé qu'il en a fait, et que je n'y ai rien trouvé que de conforme à ce que j'avois vû.

A Williamſburg, le 18 Fev. 1782.
(Signé) Thomas Bentley.

N° XL.

Conjectures concerning Wind and Water-Spouts, Tornados and Hurricanes. Communicated by Dr. JOHN PERKINS, *of Boſton, to* JOHN MORGAN, *M. D. of Philadelphia, Profeſſor of the Theory and Practice of Phyſic; and F. R. S. London, &c.*

WITH reſpect to water-ſpouts what I am about to conſider is whether water aſcends or deſcends in

theſe bodies? A queſtion which it is reaſonable to think ſhould be determined by facts, and the nature of things; and concerning which, if we wiſh to attain to any certainty, we muſt be careful not to be miſled by ſuch appearances and imaginations, as have hitherto commanded the general belief.

Agreeable to this method of inquiry, I ſhall in the firſt place produce the obſervations of three or four perſons, in whom I can confide for ſimplicity and honeſty of intention.

The firſt is that of captain Melling, formerly of Boſton, who informed me that in a voyage from our Weſt-India iſlands, in the month of Auguſt, in a warm day juſt at evening, a ſpout fell cloſe by the veſſel, and in two or three ſeconds of time came acroſs the ſtern where he then was. A flood of water, as he expreſſed it, poured upon him and almoſt beat him down, ſo that he was obliged to lay hold of what was neareſt to him, to prevent being waſhed overboard, which in his fright he was apprehenſive of. But the ſpout immediately paſſed off with a roaring noiſe into the ſea. I aſked him if he taſted the water? Taſte it ſaid he! I could not help taſting it, it ran into my mouth, noſe, eyes and ears. Was it then freſh or ſalt? as freſh, ſaid he, as ever I taſted ſpring water in my life.

The next account I had was from captain John Wakefield, alſo of Boſton, which was, that being juſt within the ſtraits of Gibraltar, a ſpout fell cloſe by his ſhip with a great roaring which he heard as he was ſetting in the cabbin, the men upon deck immediately crying out for him to come up, which he inſtantly did, and ſaw it travelling away before the ſhip, ſo near that he plainly ſaw the water deſcend. His men aſſured him that it did ſo from the beginning. He told me the wind was very ſmall during the operation of it.

Captain John Howland, of the ſame town, told me that in paſſing the calm latitudes, a ſpout fell ſo near that he

he evidently ſaw the water deſcend, very contrary to his former opinion concerning theſe bodies.

Mr. Samuel Spring, of the ſame town, told me that in a voyage from India, in paſſing the ſtraits of Malacca, a ſpout fell by eſtimation about fifty yards from their ſhip; the appearance of which was that of a column of water; or rather a ſtream of almoſt contiguous drops, from the cloud down into the ſea, making a great froth in the place like water falling among rocks, as he expreſſed it. He ſaid it was extremely plain that the water deſcended. One of the ſhip's crew was with him when he gave me this account, and confirmed it.

Many other accounts I have had from thoſe who have ſeen ſpouts, but ſo indeterminate as not to be worth much notice; I therefore content myſelf with the above, which ſpeak for themſelves.

In the next place I ſhall make a few remarks on Mr. Stuart's figures of ſpouts, which he took in the Mediterranean, as they are to be ſeen in the philoſophical tranſactions of London, Le Motte's abridgement; particularly on the pointing to the place of ſpattering in the water, and the great roar that attends the operation of a large ſpout; the buſh about the foot or baſe of a great ſpout; the break or partition in the trunk of it at the top of the buſh; and the pillar-like appearance within the buſh.

Firſt I ſhall endeavour to give ſome idea of the nature and cauſe of the pointing by the external and apparent means that nature uſes in the production of a ſpout; for as to the intimate operations of nature our faculties cannot reach them. Two or three obſervations I ſuppoſe will readily be granted, and ſhorten my work.

One is that thoſe places where the lower region of air is drawn away on one or both ſides, either by the heat of neighbouring continents, or in the calm latitudes, from which it paſſes away into, and for the ſupply of the equatorial

torial expence, are likely to be the places moſt liable to ſpouts.

In the next place I expect it will be granted that the air is much colder in the upper regions, and of conſequence ſpecifically heavier than that near the ſurface, by which when there are little or no differing motions of the air, (i. e. winds) in or about the region of the clouds, particular ſpots of air and vapour in the cloud, may be diſpoſed to deſcend, and, when ſo, will take very aptly a particular channel downwards. Theſe things being granted what is of a like kind will readily be ſo diſpoſed too, as when the atmoſphere is full of vapours condenſing into clouds, this condenſation may be quicker in one place than in another, which by the acquired cold will become more weighty and preſs moſt in a particular point. Thus it may deſcend through the more rarified and yielding ſubjacent region, the firſt drops piercing and making a channel may facilitate the deſcent of the vapour, till it puts on what Stuart calls a ſword-like appearance. The agitation cauſed by deſcending will accelerate condenſation, which together with the drops paſſing through the vapour in this channel, may at every ſtop in the paſſage be waſting the vapour, by taking it up into leſſer maſſes of water till it ends in a point, which it will in this caſe naturally do, becauſe the ſwifteſt motion down is in the center of the pointing body.

Such a ſpout may increaſe ſo as to form maſſes of water, the ſubſtance of the cloud, all obſtacles removed, paſſing down in greater abundance, and ſtill more ſwiftly condenſing; or it may preſently ceaſe when it has but juſt appeared, or inſtead of this, make, as it were, ſeveral attempts for completing a ſpout, the vapour teat advancing and retiring alternately, but which finally fail, without producing effect. Thus it has done, as it ſeems, when the cloud has not had ſufficient ſupplies for it to ſucceed in a complete and opaque ſpout. Such are the appearances of Mr. Stuart's figures, &c. The obliquity of the pointing

pointing is owing to the courſe of the air, as the bend is to two different ones at different heights.

The next thing propoſed to be conſidered, was the great roar that attends a complete ſpout while it laſts; and it is the ſame as that in cataracts or falls of water from great eminencies. This kind of roar could not exiſt in any way of aſcent, being very different from that of a whirlwind, which is no other than that of any other ſtrong wind.

Mr. Stuart's figures of the great ſpouts are drawn with the appearance of a buſh round their baſes: The caſe is ſuch, that great falls of waters muſt make a proportionable ſpray; ſo that the appearance is natural, and indeed a neceſſary conſequence. It riſes up from the foot of the ſpout and falls back in a parabolic manner into the ſea. As was ſaid of the roar juſt now, ſo it may be ſaid of this, that it could not have exiſted in any conceivable way of aſcent; while on the contrary it is perfectly agreeable to nature on the principle of deſcent. It continues the whole time of a large ſpout, increaſing and diminiſhing as that does.

The appearance of a break or partition in the trunk of the ſpout, at the top of the buſh, is a very curious phenomenon: It is not real but apparent, and could not have happened without the buſh; it being cauſed by a refraction of rays from the drops that conſtitute the top of the buſh; whence a divergency and ſo much loſs of viſion.

In great ſpouts there is alſo a pillar-like appearance, being a part of the trunk within the buſh, and by another refraction through the ſide of the buſh; by which it appears much bigger than it is, and limited in altitude by the break. The three laſt are agreeable to the laws of optics; and all the five particulars being attendants on the greater or the ſmaller ſpouts, are to me undeniable evidences of the univerſal deſcent of waters in theſe bodies. I paſs from Mr. Stuart's figures to that of Mr. Maine, which is not leſs curious.

Mr.

Mr. Maine, in the ſame philoſophical tranſactions, has given us the figure of a ſpout that fell at Topſham, near Exeter. He has depicted it in the act of ſtriking a boat as it paſſed a creek; from the bottom of which he has drawn a rebound of the whole body of the ſpout projected from it to a large diſtance; evidently proving the deſcent: And which, while he is arguing for the aſcent, it would have much become him to have accounted for, and to have ſhown how it agreed with the doctrine of aſcent. The ſpout proceeding paſſed on to the land, and brake off the limbs of a tree, beat the thatch off of a houſe, and did perhaps various other damage; but we hear nothing of its carrying up any of the light ſubſtances and dropping them at great diſtances, far from any environs of the place, which it would moſt certainly have done had there been a whirlwind, or any ſupernal ſuction employed in the operation.

The ſupernal ſuction which ſome have mentioned I ſuppoſe I may paſs over without more than the bare mention of it, but whirlwinds we know there are frequently, and ſome of conſiderable ſtrength; ſo that it being the general opinion that ſpouts are formed by them, it may not be amiſs to examine a little what force they may reaſonably be allowed to have, and the limits of it.

Their genuine cauſe, ſuppoſing them to be natural productions, is no other than the aſcent of the heated and conſequently lighter air, at the ſurface, into, or through the colder and conſequently heavier regions of the atmoſphere above: And in proportion to the different degrees of heat in one of theſe, and cold in the other, may the ſtrength of theſe be, but no more.

Dr. Arbuthnot, in his treatiſe on the air, tells us, that the rarification of the air in the hotteſt day in ſummer renders it but one-tenth lighter than that of the coldeſt in winter, or in words to this purpoſe, if I remember right, for I have not his book by me. Suppoſing then the upper region the ſame at all times as the lower one in winter

ter when a whirlwind happens, it cannot have any greater force than the weight of one-tenth of the atmoſphere, and conſidering the reſiſtance to its riſing which it muſt encounter, and the friction by the way, not ſo much; by which the ſtrength may not be equal to three feet of water. It is undoubtedly nine parts in ten too weak to make a vacuum, and having a column of water two miles high to ſupport, beſides the additional neceſſity of ſtill more force to drive it ſwiftly up, would require an atmoſphere two thouſand times more weighty than ours to raiſe water to the clouds.

Mr. Stuart ſays he ſaw the water aſcend in the heart of a ſpout; which ſeems to have been an unlucky expreſſion. The bodies of large ſpouts are too groſs and opaque for any one to ſee to the center of them; and no one has ever pretended to have ſeen water aſcend in the ſmall ones. His imagination therefore muſt have been too ſtrong for any one to confide in, ſo far as was he prejudiced; and at leaſt one of his views was to prove the aſcent; which, had he underſtood nature in a tolerable degree, he would have renounced.

That there is a gyrating appearance in the great ſpouts, ſeems to have been matter of obſervation; nor is there any improbability in the thing. As air paſſing up in whirlwinds, ſo water, or air, paſſing down may gyrate; and no doubt it does. The caſe is, that ſome have imagined the gyration to have been upwards: but the appearance of gyration up or down may eaſily deceive, as any one may be convinced by obſerving the ſwift turning of artificial ſcrews, in which the direction will appear as the perſon is diſpoſed to fancy it.

We are told the Chineſe ſailors anſwer to the queſtion, what are you afraid of in ſpouts, is, that they may break in their decks. Which ſhows they take them to be deſcents; and their knowledge is from obſervation and experience.

I conclude with one ſhort remark, viz. That to believe water aſcends in theſe bodies, to the region of the

clouds, is virtually to admit of real and eſſential miracle, without ſufficient proof; and contrary to every idea we can form, of a divinely wiſe intention.

Tornados and hurricanes I take to be of the ſame general nature, although differing in ſome circumſtances and appearances.

By the term tornado, or wind-ſpout, I mean a violent wind which has been obſerved in theſe northern colonies a few times ſince they were diſcovered and ſettled by our people. But perhaps no part of the terraqeous globe is entirely free from ſomething of the like kind, as the atmoſphere is every where liable to ſimilar commotions.

The Spaniſh term of tornado, ſeems to have been chiefly uſed for a violent ſtorm at ſea, of larger extent than what I am about to explain, which is of a more contracted nature, and confined to a narrow ſphere of action; ſo that it requires a particular and ſignificant name, ſuch as wind-ſpout, till a more ſuitable one is found for it.

Deſcription of one. It begins of a ſudden; more or leſs of clouds having been drawn together, a ſpout of wind coming from it ſtrikes the ground in a round ſpot of a few rods or perches diameter, with a prone direction, in the courſe of the wind of the day, and proceeds thus half a mile or a mile. The proneneſs of its deſcent makes it rebound from the earth, throwing ſuch things as are moveable before it, but ſome ſideways from it. A vapour, miſt or rain deſcends with it, by which the path of it is marked and wet.

I ſhall produce the inſtance of that at Leiceſter, a town about fifty miles from Boſton, a few years ſince, which being more violent than uſual, may give ſome idea of the thing.

It happened in the month of July, on a hot day about four o'clock P. M. A few clouds having gathered weſtward and coming over head, a ſudden motion of their running together in a point being obſerved, immediately a ſpout of wind ſtruck the ground at the weſtern end of a houſe

houſe, and inſtantly carried it away, with a negro fellow in it, who was afterwards found dead in the path of it. Two men and a woman, by the breach of the floor, fell into the cellar; one man was driven forceably up into the chimney-corner. Theſe were preſerved, though much bruiſed; they were wet with a vapour or miſt, as were the remains of the floor and the whole path of the ſpout.

This wind raiſed boards, timbers, &c. and carried them before it. A joiſt was found on one end driven near three feet into the ground. I imagine the ſpout took it in its elevated ſtate and drove it forceably down. By what I can learn of its procedure, it continued but three or four ſeconds of time in a place, paſſing along with the celerity of a middling wind, conſtantly declining in ſtrength till it ceaſed.

There ſeems to have been ſuch a guſt as this at cape Cod, about forty years ago, of which I received an account from two men who lived in the neighbourhood of the place. It came on of a ſudden, and was ſo violent that it threw down a young woman who happened to be in the way of it; ſhe was forced to lay hold on the buſhes which happened to be within her reach, to prevent her being carried away by it. It paſſed a pond of water, and the people wondered it did not ſuck up the water, as they conceived it to be a water-ſpout, but it did not. The young woman was however wet with the vapour that accompanied it.

Of Hurricanes, particularly thoſe of the Weſt-India Iſlands.

To account ſatisfactorily for theſe convulſions of our atmoſphere, requires a greater number and more circumſtantial obſervations than we are at preſent furniſhed with; ſo that all that can at preſent be ſaid of their origin and cauſes muſt be very conjectural. However, ſince an attempt to explain them may give occaſion to further and more exact obſervations, I ſhall proceed to offer my preſent thoughts concering them.

I believe thoſe of the Weſt-India iſlands to be owing to ſome occaſional obſtruction in the uſual and natural procedure of the equatorial trade. This I conjecture from the more than uſual preceding calms. In the natural courſe of this trade the air riſes up in the line and paſſes off towards the poles, and, in the more contracted degrees of the greater latitudes, proves the courſe of their weſtern trades: So that could this aſcent be prevented through the whole circle of that zone, there would be no more weſterly winds in theſe latitudes than any others.

Over violent rains and cold naturally tend to check the aſcent of air out of this circle, rather making it deſcend. And as there are annual rains in the equator over againſt thoſe iſlands, and in ſome years more than others, it is eaſy to conceive ſuch an effect, and the conſequences. Great clouds and over-much vapour generate cold and weight, while at the ſame time the rains are beating down the air; and as theſe prevent the riſing of the air out of the line, ſo they hinder its uſual progreſs to it from the tropics on both ſides. Thus calms muſt take place; by which the natives uſed to predict approaching hurricanes, without underſtanding the reaſon of the thing.

Much of calms in the inter-tropical climates cauſe rarifactions, and aſcents of air into the upper regions, inſtead of its being carried to the line to be diſpoſed of in the grand circulation of the atmoſphere; this will be the caſe more eſpecially among the iſlands, which increaſe the heat of the atmoſphere. Then by theſe aſcents there will be accumulations of air above, which becoming cold in the higher regions will acquire a greater ſpecific weight, and be diſpoſed to deſcend on the firſt giving way of the more rarified and yielding ſubjacent region; and this will be the caſe when there happens not to be ſufficient motions of air in the middle region to keep ſmooth and even the ſtratums of the more and the leſs rarified regions; and ſo prevent particular portions and places from bending downwards;

wards; and it is this alone that does prevent it. By a failure in this, a deſcent once begun, the conſequences cannot be prevented: The heavy quantity above will continue to deſcend till all the upper cold regions are exonerated to many hundreds of miles round; and all their contents ſhifted into the place of the rarified and lighter air below.

Such are my ideas of the cauſes and operations of a hurricane in thoſe climates. I have only to add here, that the rains in theſe violent ſtorms are, as I think, a ſtrong confirmation of the doctrine of deſcent; as they are in that kind of hurricane called by ſailors the Ox's Eye, on the coaſt of Guinea; and the like happens under various names in different parts of our globe. Even the wind in our thunder-guſts is from deſcent; the air in the cloud being rendered denſe and weighty, deſcends, and flows in the direction of the wind of the time, and with the more violence, by the warm air at the ſurface giving way to it. Theſe are ſometimes ſtrong, but ſeldom attended with danger or damage.

What objections may be raiſed againſt theſe opinions, ſhall be candidly attended to; in the mean time there is one objection that muſt be obviated, the argument being ſomewhat intereſted in it. It is as follows.

Having expreſſed my opinion that hurricanes and tornados or wind ſpouts have the ſame general nature, while we ſee a great diſparity in their magnitude and procedure; ſome explanation ſeems neceſſary to prevent miſtakes; I think a little conſideration of the place, climate, and circumſtances may remove the difficulty.

The earth is an oblate ſpheroid, its diameter many miles greater at the equator than at the poles, cauſed by its diurnal centrifugal force. If this then has ſo great an effect on terraqueous matter, it cannot have leſs on our air, but if any difference, rather more; eſpecially if we conſider, that the atmoſphere makes a larger diameter, and yet revolves

volves in the ſame time, ſo that its centrifugal force muſt be proportionably greater. The diurnal motion of the earth tends to throw a vaſt ſurplus of air on the equator, by which there is probably more air between the tropics than on the reſt of the globe. But this is a matter of conjecture not to be perceived by any ſort of preſſure any more than by the barometer, for reaſons obvious to thoſe converſant in the nature and effect of the ſeveral principles. However it might not be amiſs to obſerve whether there be any difference in the height of the mercury before any of theſe ſtorms. But to return.

Although the air in the intert-ropical latitudes is in the general lighter than in the remote ones, yet when the upper air has obtained a paſſage downward, it being vaſt in quantity, and occupying great ſpace, it will be long in accelerating and paſſing down. The paſſage is long, ſo that it will gain a great deal of the force we find it has by the length of deſcent. Neither will the middle region be diſpoſed to ſhut up without a briſk wind in it, before the whole, even to remote regions, is diſcharged through the large hiatus, as before mentioned and now repeated, to account for the duration and extent of theſe otherwiſe wonderful winds, with ſuch unrelenting violence.

Far different is the caſe of the high latitude tornados in their circumſtances and their manner, although agreeing in their general nature. The centrifugal force here has extremely little effect, unleſs to caſt the atmoſphere toward the equator inſtead of raiſing or increaſing its quantity over any given place on either ſide. Beſides there is the attraction of the ſun, moon, and all the other planets for ever within the tropics attracting the atmoſphere that way and leſſening the height of the high latitude atmoſphere, which therefore may be ſuppoſed not a fourth ſo high from the ſurface as that.

Since then the atmoſphere is vaſtly leſs in height, and alſo much leſs in quantity than toward the line, the deſcents muſt

muſt naturally be very different. Here are no accumulations aloft. The quantity ready for a diſcharge downward is vaſtly leſs, and the paſſage narrow and contracted; and by the almoſt conſtant motions of air, were there more ſupplies it would ſoon ſhut up. Beſides there is little aptneſs to flow from ſurrounding regions by reaſon of the ſmallneſs of their depth, &c. And yet ſo great is the ſpecific weight of what deſcends, that the firſt aſſault has been known to equal the greateſt violence of the proper hurricanes in their moſt powerful moments.

N° XLI.

The whole Proceſs of the Silk-Worm, from the Egg to the Cocon; communicated to Dr. JOHN MORGAN, *Phyſician at Philadelphia, in two Letters from Meſſrs* HARE *and* SKINNER, *Silk Merchants in London, July* 27, 1774, *and February* 24, 1775.

Read July 8, 1775.

IT is ſome time ſince we were honored with your eſteemed letter of 27th September laſt. We ſhould not have delayed ſo long acknowledging its receipt, if it had been in our power to have ſent you before this time the manuſcript you will receive herewith; but it is only lately we have been able to procure it from one of the firſt houſes in Italy. It contains an exact account of the Italian moſt improved method of making raw ſilk. We flatter ourſelves it may prove of ſome ſervice to your new eſtabliſhed manufactory, for whoſe uſe ſolely we ſent for it to Italy.

The large quantity of raw ſilk that continually arrives from China every year, being moſtly of a round or large ſize, will a good deal interfere with the ſale of yours, provided

provided you make it of the ſame; therefore we by all means recommend your reeling yours of the fineneſs of five to ſix cocons, no coarſer at any rate if avoidable. And we further beg leave to recommend your giving orders to your workmen to be extremely careful in aſſorting the ſilk, obſerving that all that is put into one parcel be exactly, if poſſible, of the ſame fineneſs; for if it is not, it will very much prejudice its ſale; a neglect in this particular is complained of in all the ſilk that has hitherto been received from America. If the ſilk, which was very good in itſelf that we received from Georgia, had been properly aſſorted, we certainly ſhould have ſold it 1/6 or 2/. per *lb.* better than we did. If you reel your ſilk fine the China ſilk rather promotes its ſale than otherwiſe, as it is neceſſary to have fine ſilk to work up with that of China.

We ſhall at all times be very ready to communicate to you any intelligence in our power.

We are, with reſpect,
Sir,
Your moſt obedient ſervants,
HARE & SKINNER.

CHAP. I. *Of the Silk-Worm.*

THE perſon who purpoſes raiſing a quantity of ſilk-worms, and preſerving good eggs, muſt begin a year before hand. He muſt chooſe a certain number of good cocons, or ſilk cods, the ſuperficies of which, he ſlightly pierces with a needle and thread and ſtrings them by ſcores; which done, he hangs them up in a convenient room, this being the moſt proper poſition for them. After the moths or butterflies contained in the cocon, have eaten their way through their natural incloſure, (which is generally about four days after the cocon is finiſhed*) you may

* It happens ſometimes the butterfly is longer before its birth, i. e. from 15 to 30 days if the weather is chilly. They generally come out in the morning.

may place them on a linen cloth diſpoſed vertically, as againſt a wall, or on a line, &c. where they couple and are joined during twenty-four hours. This over, the female lays her eggs during other twenty-four hours; after which ſhe dies, as does the male; this their ſecond life, if I may be permitted the term, is only of forty-eight hours duration. When the eggs are new laid, they are about the bigneſs of a common pin's head, and of a ſtraw colour; by degrees they become black, aſſume more ſolidity, loſing at the ſame time part of their bulk.

When they are arrived at this point, you muſt ſeparate them from the cloth; to effect which, you muſt dip them into a large pan filled with one half water and the other half wine, rather more than lukewarm; when your cloth has ſoaked in this liquor a little while, you may ſeparate them from the cloth with a ſilver ſpoon and dry them in a ſunny place, and take them away when they begin to be whitiſh.

When you have thus detached your eggs, you muſt keep them till the next year in a cool damp place to preſerve them from hatching during the great heat, which would ruin the project.

On the arrival of the ſpring, you muſt obſerve when the mulberry tree begins to put forth its leaves, which muſt be your ſignal to expoſe your eggs in a very warm place, that they may all hatch at once, otherwiſe they would only hatch by little and little, and in proportion as each individual would be arrived at the point of its natural maturity. In which caſe the pains required to ſeparate their different claſſes would be exceſſive, not to ſay impoſſible. To hatch your eggs you muſt carry them about you nine or ten days, keeping them in your boſom, or other parts near the body; in the night you may put them between the matraſſes of the bed. You may likewiſe hatch them by the heat of an oven, but this method is dangerous, becauſe you may poſſibly burn the worm

contained in the egg, and thereby deſtroy all your future hopes.

II. The worm is entirely black at its birth, and is about as long as an ant. He is rolled up in the egg, which otherwiſe could not contain him. He preſerves this black colour eight or nine days. After your worms are hatched you muſt put them on wicker ſhelves, which are covered firſt with paper and afterwards with a bed of the youngeſt and moſt tender mulberry leaves; you may place ſeveral ranges of them in the ſame chamber, one above another, provided you leave at leaſt a foot and a half between each range; that the ſcaffolding be in the middle of the room, and that your wicker ſhelves be not too broad, but juſt ſo as to reach on each ſide conveniently to the middle. By degrees the worm grows and requires more room. It muſt be your care to thin them, and keep thoſe of the ſame ſize as near as you can on one row; for which reaſon you muſt always leave ſome ſhelves vacant for that purpoſe.

The worm continues feeding during eight days after its birth, at the end of which he has three lines in length or the fourth part of an inch. He is then attacked with his firſt ſickneſs, which conſiſts in a kind of lethargic ſleep for three days together, during which ſpace he changes his ſkin, ſtill preſerving the ſame bulk.

This ſleep being over, he begins to eat again during five days, at which term he is grown to the ſize of ſeven lines in length, after which follows a ſecond ſickneſs*, in every reſpect like the former. He then feeds during other five days, and is now about nine lines in length, when he is attacked with his third ſickneſs; which over, he continues to eat again five days more, which are followed by his fourth ſickneſs, at which time he is arrived at his full growth, i. e. about fourteen lines in length and two in diameter.

He

* You muſt obſerve that theſe ſickneſſes are much longer, and laſt ſeven or eight days when the weather is cold.

He then feeds during five days with a moſt voracious appetite; after which he diſdains his food, becomes tranſparent a little on the yellow caſt, and leaves his ſilky traces on the leaves where he paſſes; theſe ſigns denote that he is ready to begin his cocon.

You muſt then furniſh him with little buſhes of heath, broom or other like twigs, ſticking the bundles upright in rows between the ſhelves, and forcing them a little that they may not fall; he remains ſtill two days to climb up the twigs, and ſettle himſelf on a good place, after which he begins to lay the foundation of his lodge, and is five days in ſpinning his cocon. He remains generally about the ſpace of forty-ſeven days.

III. You muſt keep your worms in a dry place, ſheltered and ſhut up cloſe, provided it be not too hot. If the weather be cold you muſt make a ſmall fire. When you furniſh them with leaves, take great care that they be thoroughly dry and ſtrew them lightly over your worms. You muſt obſerve to take away their dung very frequently. When the worms are ready to mount (in order to ſpin) if the weather be ſtifling hot attended with thunder, you will ſee them in a languiſhing condition; your care muſt then be to revive them, which is effected thus.

Take a few eggs and onions and fry them in a pan with ſome ſtale hog's lard, the ranker the better, and make pancake; which done, carry it ſmoaking hot into the room where they are kept, and go round the chamber with it. You will be ſurprized to ſee how the ſmell revives them, excites thoſe to eat who have not done feeding, and makes the others that are ready to ſpin, climb up the twigs.

Theſe little creatures require a great deal of care in the management; one or other muſt attend them day and night; you muſt be very dexterous and gentle in handling them; and, as I may ſay, the whole ſucceſs depends on the care you obſerve and pains you take in rearing them.

The worms cannot ſuffer ſtrong ſmells, ſuch as tobacco

and the like, for which reaſon you muſt avoid offending their delicate organs.

In many parts of Italy, amongſt others Romagna and La Marche of Ancona, they have two ſilk racoltas, or harveſts. They keep the eggs in very cool places, and when the mulberry tree begins to bud again (for during the racolta it is ſtripped of its leaves for food for the worms) they expoſe their eggs to hatch. Sometimes they give roſe leaves to the young worms, when there are no young mulberry leaves. The cocons of this ſecond racolta are rather inferior to thoſe of the firſt. The ſilk worm is generally fourteen lines in length and two in diameter and ſix and two-ſevenths in circumference. He is either of a milk or pearl colour or blackiſh: theſe laſt are the beſt. His body is divided into ſeven rings, to each of which are joined two very ſhort feet. He has a ſmall point like a thorn, exactly above the anus. The ſubſtance which forms the ſilk, is in their ſtomach, which is very long; wound up as it were on two ſpindles and ſurrounded with a gum, commonly yellow, ſometimes white, not often greeniſh. When the worm ſpins his cocon, he winds off a thread from each of his ſpindles, and joins them, afterwards, by means of two hooks which are placed in his mouth; ſo that the cocon is compoſed of a double thread. Having opened a ſilk worm you may take out the ſpindles which are folded up in three plaits, and on ſtretching them out and drawing each extremity, you may extend them to near two ells in length. If you then ſcrape the thread ſo ſtretched out with your nail, you will ſcratch off the gum, which is very much like bees-wax, and performs the ſame office to the ſilk it covers, as a gold leaf does to the ingot of ſilver it ſurrounds, when drawn out by the wire-drawer; the ſilk then remains of a pearl colour. This thread which is extremely ſtrong and even is about the thickneſs of a middling pin.

Three things very remarkable in this inſect, are,

1. They

1. They deſcribe a ſemicircle in eating.

2. Their excrement has perfectly the form of a mulberry.

3. They have no ſex before their metamorphoſis.

CHAP. II. *Of the Cocons.*

I. IT is almoſt a general rule to wait ſix or ſeven days after all the cocons ſeem to be formed, before you take them off the boughs in order to give the worms time to bring them to perfection. It is then proper from that time to give ſome air to the room in which you have kept them, in order to diſſipate a conſiderable dampneſs which the worms exhale on their mounting, (when they have not been well fed and kept, for when they have been properly nurſed this dampneſs is not to be found) and which is of great detriment to the cocons, either by rotting them, rendering them ſoft, or covering them with ſpots.

The cocons may be divided into two general claſſes, the white and the yellow, in the yellow you meet with all the ſhades from a bright yellow diminiſhing at laſt to white, ſome few are of a pale green. We reckon nine ſorts of cocons, viz.

1. The good cocons are thoſe which are brought to their perfection, ſtrong and little, and not at all ſpotted.

2. The pointed cocons are thoſe, one of whoſe extremities riſes up in a point. After having afforded a little ſilk, the point, which is the weaker part, breaks or tears, and it is impoſſible to continue winding that cocon any longer, becauſe when the thread comes round to the hole it is of conſequence broke.

3. The cocalons are a little bigger than the other, yet they do not contain more ſilk, becauſe the contexture is not ſo ſtrong. In winding they are to be ſeparated from the reſt, becauſe they require to be wound in cooler water, otherwiſe they furze out in winding.

4. The

4. The dupions, or double cocons, are ſo called becauſe they contain ſometimes two and ſometimes three worms, who have jointly formed one ſingle cocon. They interlace their threads, for which reaſon they are to be kept aſunder from the reſt; they make the ſilk we call dupions.

5. The ſoufflons are cocons very imperfect, whoſe contexture is looſe, ſometimes to that degree that they are tranſparent, and bear the ſame proportion to the others, as a gauze to a ſattin. Theſe cannot be wound.

6. The perforated cocons are ſo called, becauſe they have a hole at one end, for which reaſon they alſo cannot be wound.

7. The calcined cocons are thoſe whoſe worm, after the formation of the cocon, is attacked with a ſickneſs which ſometimes petrifies it, and at others reduces it to a fine white powder, without in the leaſt endamaging the ſilk; on the contrary, theſe cocons produce more ſilk than the others, becauſe the worm is conſiderably lighter. They are to be diſtinguiſhed by the noiſe the petrified worm makes when you ſhake the cocon. In Piedmont they ſell for half as much again as the others. It is very rare to ſee a parcel of 25 *lb.* of them at a time: 63 *lb.* of theſe cocons have produced 1 *lb.* 1 *oz.* of fine ſilk of five to ſix cocons.

8. The good choquette conſiſts in thoſe cocons whoſe worm dies, before he has brought it to its perfection. They are to be known by the worms ſticking to one ſide of the cocon, which is eaſily to be perceived when on ſhaking it you do not hear the chryſalis rattle. Theſe cocons are of as fine ſilk as the others, but they are to be wound ſeparately becauſe they are ſubject to furze out, and the ſilk has not ſo bright a colour, neither is it ſo ſtrong and nervous.

9. The bad choquette is compoſed of defective cocons, ſpotted or rotten. They wind many of theſe cocons together. It makes a very foul bad qualified ſilk of a blackiſh colour.

II. To

II. To know whether a cocon be good or not you muſt obſerve if it be firm and ſound, or not, if it has a fine grain, and if the two ends are round and ſtrong. The cocons of a bright yellow yield more ſilk than the others, becauſe they contain a greater quantity of gum; but the advantage accrues to the winder only, becauſe all this gum is loſt in the dying. For which reaſon, as well as for certain colours they take better, the pale ſilks are preferred, becauſe having leſs gum they loſe leſs in boiling.

In the number of cocons that are bought, there ought to be neither ſoufflons, nor perforated cocons; becauſe the ſeller is obliged to keep them apart and to ſell them as ſuch; notwithſtanding which, you may always reckon on half profit of theſe ſorts that remain with the others, and if to theſe you add the dupions and choquette, you may calculate them at ten per cent.

The cocons of the mountains are better than thoſe of the plain; there is a greater quantity of white amongſt them. 'Tis true they are not ſo large as thoſe of the plain, but the worm, at the ſame time, is proportionably leſs. The reaſon of which is, that the air of the mountains being ſharper, the worm labours with greater vigour. They ſucceed, likewiſe, better in the dry plains than in the damp and marſhy parts, becauſe the leaf is more nouriſhing. Five or ſix days after the cocon has been detached from the branches, it is your buſineſs to prevent the birth of the worm, who would, otherwiſe, pierce through the ſhell, and thereby render the cocon uſeleſs. To prevent which you muſt put your cocons in long ſhallow baſkets, and fill them up within an inch of the top. You then cover them with paper and a wrapper over that. Theſe baſkets are to be diſpoſed in an oven, whoſe heat is as near as can be that of an oven from which the bread is juſt drawn after being baked. After your cocons have remained therein near an hour, you muſt draw them out, and to ſee whether all the worms are dead, draw out a dupion from the

the middle of your baſket and open it, if the worm be dead, you may conclude all the reſt are ſo; becauſe the contexture of the dupion being ſtronger than that of the other cocons, it is conſequently leſs eaſy to be penetrated by the heat. You muſt obſerve to take it from the middle of the baſket, becauſe in that part the heat is leaſt perceptible; after you have drawn your baſkets from the oven, you muſt firſt cover each of them with a woolen blanket or rug, leaving the wrapper beſides, and then you pile them one on the other. If your baking has ſucceeded, your woolen cover will be all over wet with a kind of dew, the thickneſs of your little finger. If there be leſs, it is a ſign your cocons have been too much or too little baked. If too much baked, the worm being over dried, cannot tranſpire a humour he no longer contains, and your cocon is then burnt. If not enough baked, the worm has not been ſufficiently penetrated by the heat to diſtil the liquor he contains, and in that caſe is not dead.

You muſt let your baſkets ſtand thus covered five or ſix hours if poſſible, in order to keep in the heat, as this makes an end of ſtifling thoſe worms, which might have avoided the firſt impreſſion of the fire.

You are likewiſe to take great care to let your cocons ſtand in the oven the time that is neceſſary; for if they do not ſtand long enough your worm is only ſtunned for a time and will afterwards be revived. If on the other hand, you leave them too long in the oven you burn them, many inſtances of theſe two caſes are frequently to be met with.

It is a good ſign when you ſee ſome of the butterflies ſpring out from among the cocons which have been baked, becauſe you may be certain they are not burnt. For if you would kill them all to the laſt worm you would burn many cocons, which might be more expoſed to the heat than that particular worm.

III. When

III. When you put your cocons into the oven, you muſt be very careful in picking out all the ſpotted ones, otherwiſe they communicate their ſpots by the great perſpiration occaſioned in them by the heat. If you have a parcel of ſtrong and another of weak cocons, and you can only wind a part of them freſh (i. e. without baking) give the preference to the weak cocons, and bake your ſtrong ones, becauſe the latter, containing more gum, ſupport the baking much better and ſuffer leſs than the weak ones.

As faſt as the cocons you buy are brought in, put them in baſkets and expoſe them to the ſun, if it ſhines, in caſe your oven be full, in order at leaſt to ſtun the worm and prevent his working to pierce his cocon during that time.

It is very proper likewiſe that they be a little in the air before you put them in the oven; becauſe the peaſants bring them in baſkets heaped one on the other, which heats them and renders them extremely ſoft, but the air brings them to their proper tone again.

Sometimes the peaſants ſell you the cocons ready baked when they have been obliged to keep them ſometime. It is eaſy to know them, becauſe the worms when baked, being dry, make a louder noiſe on rattling them than when they are freſh.

When your cocons are fully baked, and have ſtood long enough, you muſt ſpread them half a foot thick on broad ozier ſhelves, which are diſtributed into as many ſtories as the height of the room will admit of, two or three feet diſtant one from the other; taking care to turn them every day, and to change their places, for otherwiſe there are many inconveniencies that would ariſe from ſuch a neglect. They would become mouldy and the moths would eat them. Beſides this, it is abſolutely neceſſary in order to ſeparate the ſpotted cocons, or the bad choquette, which would ſpread to all the cocons that are near them, and muſt be wound immediately to prevent their damaging any further.

The building where you ſpread your cocons is called the Coconiere, and conſiſts of one or more large rooms, in which are diſtributed as many ranges as you can conveniently place, taking care that the ſupporters touch neither the roof nor the wall, becauſe if there were any rats in the Coconiere they would come down the poles, and deſtroy the cocons, they being very greedy of the worm contained in them.

A middling cocon has about thirteen lines in its greater diameter, by eight lines the leſſer diameter, ſome are larger, ſome are ſmaller; but this is the general ſize. The dupion has generally fifteen lines great diameter by nine leſſer diameter.

The cocon is compoſed of ſeveral ſtrata or ſurfaces applied one on the other; notwithſtanding they all communicate, otherwiſe it would be impoſſible to wind them off. It is an eaſy matter to take off one or more of theſe ſurfaces, the uppermoſt of which is coarſer, leſs gummed, and higher coloured than the undermoſt. Finally, theſe ſurfaces are compoſed of a fine ſort of ſaliva, whoſe texture has a tolerable reſemblance to the thin ſkin you find joined to the inſide of a hen's egg.

The cocons produce a thread of a very unequal length, you may meet ſome that yield twelve hundred ells, whilſt others will ſcarcely afford two hundred ells. In general you may calculate the production of a cocon, from five hundred to ſix hundred ells in length.

IV. The worm or chryſalis, as he is incloſed in his cocon is ſhrunk up into himſelf, ſo that it is but half as long in his primitive ſtate, but it is on the contrary as thick again.

He is of a cinnamon colour, and full of liquor, rather clear, which forms the ſeed in the males, and the eggs in the females. Though he ſeems to be inſenſible in that ſtate, yet you may perceive he is not wholly ſo, for on piercing him with a pin ſlightly, you will ſee him move,

move, and we make uſe of theſe experiments to ſee if they have been killed in the oven.

The worm dries the older it grows, ſo that the ſame quantity, or the ſame number of cocons decreaſes daily in weight. The cocons which encloſe the male butterfly have more ſilk at the extremities, than thoſe which contain the females; but it is very difficult to perceive this difference, the moſt ſkilful connoiſſeurs will miſtake at leaſt twenty in a hundred.

When the worm wants to break his way through, he pierces the cocon, firſt wetting it a little in order to gnaw it the more eaſily; he has then only to ſtrip off his upper coat, under which he has another quite white, with wings.

When he comes out, his wings, which at firſt appear very ſmall, open and diſplay themſelves by little and little, and are entirely at liberty in an hour or two. As ſoon as born he ſeeks a female, and one would ſay he is born again merely to propagate his ſpecies, for he expires a very little time after having performed his function.

CHAP. III. *Of Cocons Royal, Perforated Cocons, and Soufflons.*

THE royal cocons are thoſe which you have kept for ſeed. The worm makes a hole in them for his paſſage, ſo that they cannot be wound, and are in the ſame claſs with the perforated cocons.

Neither can the ſoufflons be wound, becauſe their thread being the produce of a weak, ſick worm, it has not the gum it ought to contain. Beſides they cannot be wound off, their thread being interlaced and entangled.

The uſes you may make of theſe cocons are the following; and firſt for the

Soufflons; you muſt let them boil for about half an hour in common water, after which you muſt dry them. When they are quite dry you muſt threſh them on the

floor with a flail, to bring out the worm, which is reduced to aſhes by the fire and air. Afterwards you put them on a diſtaff and open them; to effect which you muſt take them by the two ends and ſtretch them out at arms length, you may then faſten them on your diſtaff.

2. The perforated cocons; you muſt obſerve the ſame method as for the ſoufflons, except that you muſt let them boil three-quarters inſtead of half an hour, becauſe they contain a greater quantity of gum.

3. The cocons royal. As it is natural to ſuppoſe you keep the flower of your cocons for ſeed; they are fuller of gum than the others, for which reaſon you muſt let them boil an hour; after which you muſt not threſh them as the former, becauſe they contain no worm, neither is it neceſſary to ſtay till they are quite dry before you ſpin them; on the contrary, they open more eaſily when damp. The produce of theſe three ſorts of cocons, when worked, makes what we call *fleuret*.

After you have boiled the cocons and threſhed them well, to ſhake out the worm they contain, you may card them inſtead of opening them as above, you will then make a much more beautiful fleuret, and of a brighter colour, but it will at the ſame time come conſiderably dearer, becauſe of the waſte in carding. A good ſpinſter performs a very reaſonable days work if ſhe can ſpin an ounce of fleuret.

To ſum up the whole, and give you a notion of the value of theſe three ſorts of cocons, you may calculate thus.

If the good cocons are worth one hundred, the perforated are worth thirty-three one third, the ſoufflons twenty-five, the royal cocons two hundred and fifty; but if your royal cocons are not choſen ones for ſeed, they are worth but two hundred.

The beſt fleuret is that which proceeds from the royal cocons, afterwards that of the perforated cocons unchoſen, laſt of all that of the ſoufflons.

CHAP.

CHAP. IV. *Of the Filature, or Winding from the Worm.*

Although the freſh cocons, that is to ſay, thoſe that have not been baked in the oven, yield a brighter ſilk than thoſe that have, and at the ſame time yield better weight, by reaſon of part of their gum which they have not loſt by the fire, yet moſt people prefer thoſe that are baked, in order to have a ſilk more even in its colour; unleſs you could have a conſiderable quantity of freſh cocons, and time to wind them ſo; for otherwiſe it is undeniable, that the freſh would be much more advantageous, as well for the reaſon above mentioned as becauſe they are eaſier to wind, not having been dried by the fire.

Before you begin to wind, you muſt prepare your cocons as follows.

1. In ſtripping them of that waſte ſilk that ſurrounds them, and which ſerved to faſten them to the twigs. This burr is proper to ſtuff quilts, or other ſuch uſes; you may likewiſe ſpin it to make ſtockings, but they will be coarſe and ordinary.

2. You muſt ſort your cocons, ſeparating them into different claſſes in order to wind them apart. Theſe claſſes are,

The good white cocons.
The good cocons of all the other colours.
The dupions.
The cocalons, among which are included the weak cocons.
The good choquette; and, laſtly,
The bad choquette.

In ſorting the cocons, you will always find ſome perforated cocons amongſt them, whoſe worm is already born; thoſe you muſt ſet apart for fleuret. As I have deſcribed above, you will likewiſe find ſome soufflons, but very few; for which reaſon you may put them among the bad choquette, and they run up into waſte.

The

The good cocons, as well white as yellow, are the eaſieſt to wind; thoſe which require the greateſt care and pains are the cocalons; you muſt wind them in cooler water than the others, and if you take care to give them to a good windſter, you will have as good ſilk from them as the reſt. You muſt likewiſe have careful windſters for the dupions and choquettes. Theſe two articles require hotter water than the common cocons.

The good cocons are to be wound in the following manner. Firſt chooſe an open convenient place for your filature, the longer the better, if you intend to have many furnaces and coppers. This building ſhould be high and open on one ſide and walled on the other, as well to ſcreen you from the cold winds and receive the ſun, as to give a free paſſage to the ſteam of your baſons or coppers.

Theſe coppers or baſons are to be diſpoſed (when the building will admit of it) in a row on each ſide of the filature, as being the moſt convenient method of placing them, for by that means in walking up and down you ſee what every one is about. And theſe baſons ſhould be two and two together, with a chimney between every couple.

Having prepared your reels, (which are turned by hands and require a quick eye) and your fire being a light one under every baſon, your windſter muſt ſtay till the water is as hot as it can be without boiling. When every thing is now ready, you throw into your baſons two or three handsful of cocons, which you gently bruſh over with a wiſk about ſix inches long, cut ſtumpy like a broom worn out: by theſe means the threads of the cocons ſtick to the wiſk. You muſt diſengage theſe threads from the wiſk, and purge them by drawing theſe ends with your fingers till they come off entirely clean. This operation is called la Battüe.

When the threads are quite clear, you muſt paſs four of them (if you will wind fine ſilk) through each of the holes in a thin iron bar that is placed horizontally at the edge

edge of your baſon; afterwards you twiſt the two ends (which conſiſt of four cocons each) twenty or twenty-five times, that the four ends in each thread may the better join together in croſſing one another, and that your ſilk may be plump, which otherwiſe would be flat.

Your windſter muſt always have a bowl of cold water by her, to dip her fingers in, and to ſprinkle very often the ſaid bar, that the heat may not burn the thread.

Your threads, when thus twiſted, go upon two iron hooks called rampins, which are placed higher, and from thence they go upon the reel. Now at one end of the axis of the reel is a cog-wheel, which catching in the teeth of the poſt-rampin, moves it from the right to the left, and conſequently the thread that is upon it; ſo that your ſilk is wound on the reel croſs-ways, and your threads form two hanks of about four fingers broad.

As often as the cocons you wind are done, or break or diminiſh only, you muſt join freſh ones to keep up the number requiſite, or the proportion; I ſay the proportion, becauſe as the cocons wind off, the thread being finer, you muſt join two cocons half wound to replace a new one: Thus you may wind three new ones and two half wound, and your ſilk is from four to five cocons.

When you would join a freſh thread, you muſt lay one end on your finger, which you throw lightly on the other threads that are winding, and it joins them immediately, and continues to go up with the reſt. You muſt not wind off your cocons too bare or to the laſt, becauſe when they are near at an end, the bairré, as we call it, that is the huſk, joins in with the other threads and makes the ſilk foul and gouty.

When you have finiſhed your firſt parcel, you muſt clean your baſons, taking out all the ſtriped worms, as well as the cocons, on which there is a little ſilk, which you firſt open and take out the worm and then throw them into a baſket by you, into which you likewiſe caſt the looſe ſilk that comes off in making the battüe. You

You then proceed, as before, with other two or three handsful of cocons; you make a new battüe; you purge them, and continue to wind the same number of cocons or their equivalent, and so to the end.

As I said above, your windster must always have a bowl of cold water by her, to sprinkle the bar, to cool her fingers every time she dips them in the hot water, and to pour into her bason when necessary, that is, when her water begins to boil. You must be very careful to twist your threads a sufficient number of times, about twenty-five, otherwise your silk remains flat, instead of being round and full; besides when the silk is not well crossed it never can be clean, because a gout or nub that comes from a cocon will pass through a small number of these twists, though a greater will stop it. Your thread then breaks and you pass what foulness there may be in the middle of your reel, between the two hanks, which serves for a head band to tie them.

You must mind your water be just in a proper degree of heat. When it is too hot the thread is dead and has no body; when it is too cold, the ends which form the thread do not join well, and form a harsh ill-qualified silk.

You must change the water in your bason four times a day, for your dupions and choquette, and twice only for good cocons when you wind fine silk, but if you wind coarse silk it is necessary to change it three or four times. For if you was not to change the water the silk would not be so bright and glossy, because the worm contained in the cocons foul it very considerably. You must endeavour as much as possible to wind with clear water, for if there are too many worms in it, your silk is covered with a kind of dust, which attracts the moth and destroys your silk.

You may wind your silk of what size you please, from one cocon to a thousand; but it is difficult to wind more than thirty in a thread. The nicety, and that in which consists

conſiſts the greateſt difficulty, is to wind even, becauſe as the cocon winds off, the end is finer, and you muſt then join other cocons to keep up the ſame ſize. This difficulty of keeping the ſilk always even is ſo great, that (excepting a thread of two cocons, which we call ſuch) we do not ſay a ſilk of three, of four, or of ſix cocons, but a ſilk of three to four, of four to five, of ſix to ſeven cocons. If you proceed to a coarſer ſilk you cannot calculate ſo nicely as to one cocon more or leſs. We ſay for example, from twelve to fifteen, from fifteen to twenty, and ſo on.

It is eaſy to conceive, that it is more difficult to wind a coarſe ſilk even, than a fine one, becauſe it is harder to keep a great number of cocons always to the ſame ſize, than a ſmall one.

The dupions which you deſign for rondelette, or ordinary ſewing ſilk, are to be wound from fifteen to twenty. The reſt you may wind as coarſe as poſſible, i. e. from forty to fifty: they ſerve to cover and fill up in coarſe ſtuffs, and may likewiſe be uſed for ſome ſort of ſewing ſilk.

The good choquette is to be wound according to the uſes to which you intend to apply it; however not finer than from ſeven to eight. The bad choquette you may wind from fifteen to twenty cocons.

In winding the good cocons, you will always meet with ſome defective, which will not wind off, or are full of gouts and nubs. Theſe you muſt take out of your baſon and keep by themſelves. They are called *baſſinats*. They are to be wound apart as coarſe as you can. They make a foul, dirty ſilk. To have a good ſilk, you muſt wind in fine weather. If the wind be high it ſhakes your ſilk, and prevents its lying ſmooth on the reel, forms ſtrings of threads, which make it very difficult to wind on bobbins. If the weather is rainy the ſilk is damp, and has not that luſtre it ought to have, or which it has when it dries, as it goes upon the reel. You muſt mind not to hank it when damp, but let it dry on the reel; otherwiſe it would be furzy.

I have now only to ſpeak of the waſte that comes from the battüe, and the huſks of the cocons, that have ſtill ſome ſilk upon them, which are thrown into baſkets in winding, and are what we call *moreſques*. Theſe you firſt dry in the ſun, then threſh, and afterwards card and ſpin them to make fleuret. One hundred and fifty ounces of good cocons yield about eleven ounces of ſilk from five to ſix cocons; if you wind coarſer, ſomething more. You may wind about eleven or twelve ounces of ſilk from five to ſix cocons in fourteen hours.

The ſilk which is made of baſſinats and bad choquette ſerves to make ſtockings and coarſe heavy ſtuffs, ſuch as ſattinades and damaſks for hangings, &c. &c.

N° XLII.

The Art of making Anatomical Preparations by Corroſion. By JOHN MORGAN, *M. D. Profeſſor of the Theory and Practice of Phyſic in the Univerſity of Pennſylvania, Member of the Royal College of Phyſicians at Edinburgh, and F. R. S. at London, &c.*

AS no branch of ſcience more certainly leads to an intimate acquaintance with the functions of the animal body, (which is the foundation of all rational knowledge of the cauſes and cure of diſeaſes) than that of the ſtructure of the vaſcular ſyſtem, the origin, diviſions, different ramifications and numerous inoſculations of the veſſels into, and their communication with each other, I have always thought this field of uſeful information deſerved to be cultivated with great induſtry and attention. In effect it brings us immediately, and in the moſt compendious way, to acquire a knowledge of the nature, and of the motions of the fluids which circulate through them, of their diſtribution throughout the different parts of the body,

body, and of the action and uſes of the veſſels containing, as well as of the humours contained in them. In particular, it behoves every practitioner of phyſic to ſtudy the vaſcular texture and compoſition of the viſcera, for upon their healthful action the continuance of life, free from diſeaſe, principally depends; and the more their functions are injured, the more dangerous diſeaſes are thereby generated. From a relaxation of them ariſe atonia and weakneſs, and from obſtruction of them infarctions, inflammations, tumors and ſchirri are produced. To an acquaintance with their ſtructure and anaſtomoſes, and the fluids they carry, we muſt be chiefly indebted for our knowledge of the doctrines of reſolution and ſuppuration, and for the indications that point out to the phyſician by what means to accompliſh theſe deſirable events, according to circumſtances.

The little progreſs which practical anatomy has hitherto made in America, and the great conſequence it may be of to the riſing ſtudents of phyſic and ſurgery, to employ more of their attention on this uſeful ſubject, are my motives for laying before you this eſſay, in hopes through this channel to ſtir them up to proſecute it with more zeal and ardor. This becomes the more neceſſary, becauſe, owing to the late revolution, the ſubjects of North-America having eſtabliſhed themſelves into independent ſtates, have at preſent leſs commerce and leſs intercourſe with the learned and poliſhed nations of Europe. At leaſt fewer ſtudents from America have recourſe to them for improvement in the knowledge of their profeſſion than formerly, in as much as medical ſchools and colleges have been founded in ſeveral of theſe different ſtates, ſince the author of this eſſay firſt recommended and aſſiſted in carrying into execution the plan of tranſplanting phyſic, as a ſcience, from acroſs the ocean, by inſtituting medical ſchools on this weſtern ſide of the Atlantic*.

* See his diſcourſe on the inſtitution of medical ſchools in America, delivered at a public commencement in the college of Philadelphia, May 1765.

Upon our own exertions muſt we therefore chiefly depend for building up the medical fabric, erecting uſeful temples of the healing arts, and diffuſing the lights we can kindle through this new world. I know no one ſtep that can be more uſeful to accompliſh this undertaking, than to teach the art of inveſtigating the ſtructure of the different parts of the animal body, by injections and corroſions, and other preparations of wax.

Such is the preſent ſtate of anatomy in this country that there are at preſent but very few, I believe I may ſay no ſuch preparations worth mentioning to be met with here, that have been made in America. Doctor Chovet, now reſident in this city has indeed a good collection of wax preparations, of different parts of the human body, which he made in his younger days and brought hither from Europe. But nothing of this kind has hitherto been practiſed, or it has been ſo taught as never to have been of laſting uſe to any that I know of.

Being well acquainted with the general deſire that fills the breaſts of my countrymen, to acquire and improve every kind of ſcience that is uſeful, which is properly laid before them, I truſt this attempt will ſtir up many to learn and practiſe thoſe leſſons which, for their particular benefit, I now unfold to them; nor do I doubt in a little time but we ſhall ſee ſuch an emulation kindled for improving on theſe hints, that all kinds of uſeful preparations will be ſo common after a while, as not only to give riſe to anatomical cabinets and repoſitaries for ſpecimens of the animal, vegetable and foſſil kingdoms, as will tend to throw great light upon philoſophy in general, but contribute to lay a ſolid and uſeful foundation of natural hiſtory in America.

The firſt rudiments of this art that I acquired was from the two Hunters, known through all Europe for their ſuperior ſkill in anatomy, and acting as practical diſſector to the celebrated doctors Colignon and Smith, profeſſors of anatomy in the univerſities of Cambridge and Oxford,

ford, which I further improved by practice at Paris with Monſ. Süe, to whom I am wholly indebted for my knowledge of anatomical preparations in wax.

The kind of preparations of thoſe parts of the animal body which admit of it that I now propoſe to explain, namely by injection and corroſion, exceeds in beauty, nicety and uſefulneſs, that which is commonly called diſſection.

In fact, in this latter, we can trace nature but very imperfectly, becauſe by diſſection, the larger veſſels only are preſerved from the knife, and for the moſt part all the ſmaller are unavoidably cut away.

On the contrary, in anatomical preparations by corroſion, even the very ſmall veſſels may be kept entire, and we can ſee, at a caſt of the eye, the courſe and diſtribution of all the vaſcular ſyſtem even to the ſize of an hair, called capillary veſſels, and thoſe too diſengaged from the ſurrounding parts, which otherwiſe wholly conceal, or make them difficult to be perceived. It is impoſſible that with only the aſſiſtance of a diſſecting knife, any perſon ſhould be able to lay open to view all thoſe ſmaller veſſels, however ſkilful and experienced the hand may be that directs it. The exact and perfect imitation of nature which this ſort of preparations preſents, the eaſe with which they are made, and their extraordinary beauty and neatneſs, render a knowledge of this art ſo much the more deſirable.

The art of injecting the very fine veſſels of the body with common injection, was well known to the celebrated Ruyſch, the moſt famous anatomiſt, in that way, of any living in Europe in his day; and therefore it has been ſometimes called the Ruyſchian art, but it fell ſhort of the one I now undertake to explain, becauſe in his preparations the minute veſſels only become viſible, ſo far as the ſubſtance through which they proceed was tranſparent, but our art extends to the removal of every ſurrounding

rounding ſubſtance, and leaves them entirely naked and perfectly expoſed to the eye. I once ſhewed a preparation of the veſſels of a kidney I had thus executed at Paris, to a meeting of the French academy of ſurgery in the year 1764, who allowed it to be curious and quite new to them. I think none of the members preſent at that meeting, except Monſ. Morand, ſecretary of that academy, who had been in England and was acquainted with doctor Hunter, alledged their having ever ſeen a ſimilar preparation. At their requeſt I preſented a memoir on the ſubject, and ſince that time Monſ. Süe has beſtowed one entire ſection in treating expreſsly upon it, with a polite acknowledgement of his having acquired his knowledge from me, only with the particularity of naming me as one of the faculty of Edinburgh, without taking any notice of my being a Pennſylvanian by birth, or native of America, which have led ſome into miſtakes concerning the author of that piece. The reaſon may be that Americans before the revolution, being but little conſidered in any other light than as coloniſts, their nation was ſeldom taken notice of, and I was introduced to him firſt as a graduate of the univerſity of Edinburgh, and known to him afterwards as a member of the royal college of phyſicians of that place.

Theſe preparations are, ſince that period, become common in France, and the art is now well known, and cultivated ſucceſsfully by Monſ. Süe and others; but it was unknown there till I communicated it, firſt at Paris, and afterward in the ſouth of France; where I had the honour of explaining it to the illuſtrious Monſ. Imbert, chancellor of the univerſity at Montpelier, and to Monſ. Bourgelas, principal of the Ecole veterenaire, or academy eſtabliſhed at Lyons for the improvement of the ſcience of horſemanſhip, juſtly celebrated for his very elegant preparations of the anatomy of horſes, &c. But what gave me equal pleaſure and ſurprize, was the admiration excited on my preſenting

preſenting only a part of the vaſcular preparation of a kidney by corroſion, (the reſt being broke down in a journey by land of above a thouſand miles) which was expreſſed by the celebrated Morgagni, illuſtrious profeſſor of anatomy in the univerſity of Padua. He had kept up a literary correſpondence with Ruyſch when alive, had been favoured with ſpecimens of this great man's preparations, and declared that in compariſon to the preparation I gave him, they were "*rudis indigeſtaque moles.*" From this ſmall ſpecimen, he ſaid, "ex ungue leonem," he could readily comprehend that the uſefulneſs of this kind of knowledge amongſt the learned in anatomy, muſt become great and extenſive.

I mention theſe anecdotes merely to ſhow how recent, or at leaſt how confined the knowledge of this uſeful art then was, being limited, as far as I know, to Great-Britain only. I ſuppoſe it to be owing to this circumſtance, viz. that real practical anatomiſts who have excelled in their preparations, have too generally kept ſecret the methods and arts they employed in making thoſe preparations. For this reaſon, much I think is due to the memory of the great Profeſſor Monro, of Edinburgh, who has publiſhed a paper upon the art of making injections.

So far as I can learn, this art cannot be traced farther back than to the learned Dr. Nichols of London, who formerly gave lectures in anatomy both there and at Oxford, and from whom Dr. Hunter acknowledged to his pupils that he received his firſt information. He then deſerves to be looked upon as the author and inventor of this art. When Dr. Nichols declined the buſineſs, Dr. Hunter and his brother Mr. Hunter, took up the profeſſion of anatomy. Without doubt, thoſe unrivalled brothers in anatomical ſkill, made conſiderable improvements in the art of injecting and diſſecting animal bodies; and it is likely improved the compoſition of injections for corroſion.

My

My well meant intention of marking the riſe and progreſs of this art, and of exciting an emulation in my countrymen to proſecute and improve it, will, I hope, not only excuſe but juſtify both the matter and length of my introduction.

Theſe preliminaries being thus ſettled, I now proceed to the main object of this communication. Without expatiating upon the advantages that will attend an accurate knowledge of this art, for the ſake of perſpicuity I ſhall here reſtrict myſelf to laying the following obſervations before you, reduced to general heads, in as few words, and in as conciſe a manner as I am able. They may be uſeful to thoſe who wiſh to put them in practice, and will perhaps give occaſion to perſons who are curious in making experiments in anatomy, to light up ſome new diſcovery.

The art of making anatomical preparations by corroſion, depends on the following principles.

1. We ought for the matter of injection to make uſe of a ſubſtance that is poſſeſſed of a ſuitable degree of conſiſtence, and fine enough to penetrate into the minuteſt veſſels, and which at the ſame time has ſuch a firmneſs of texture as not to alter with the changes of the temperature of the atmoſphere, that is, it ought not to be ſubject to melt with the ſummer's heat, nor to break down from its brittleneſs on being gently handled in the winter.

2. The colours to be employed for ſake of diſtinguiſhing the different orders of veſſels, whether arterial, venal, tracheal or others, ought to be of ſuch a nature as not to be changed upon application of the menſtruum.

3. We ought to make uſe of a menſtruum that is capable of conſuming the muſcular, parenchymatous, cellular or fatty parts that ſurround the veſſels, without affecting the ſubſtance that we uſe for injection to fill the veſſels.

4. Care and addreſs are neceſſary in the perſon who makes the injection.

5. Laſtly, great attention is requiſite in removing the looſe and corroded parts, and in ſeparating them from the injected

injected veſſels without breaking them down by the force applied in cleanſing them.

The following directions will ſerve to guide the operator in theſe different manœuvres.

The common injections are compoſed of wax and ſuet, or of wax and oil; the ſuet or oil is made uſe of to ſoften the wax, and to give it the neceſſary conſiſtence. We cannot employ ſuch a compoſition in our corroded preparations, being oppoſed to the third principle laid down; becauſe the menſtruum we uſe for deſtroying the parts that ſurround the veſſels, will alſo attack and conſume the fat and animal ſubſtances which enter into the compoſition of the injection. But we may employ the following compoſitions, the goodneſs whereof has been proved by repeated trials, viz.

First Receipt.

Take of white or the beſt yellow wax and purified roſin each equal parts, e. g. ten or twelve ounces; melt them together and add a ſufficient quantity of ſpirit of turpentine, to give a due conſiſtence, that is from ſix to eight ounces.

It is adviſeable to melt the roſin firſt, and ſtrain it through a piece of fine linen; becauſe, in the ſtate it is bought out of the ſhops, it is often mixed with foreign ſubſtances.

I am of opinion this injection will turn out to be finer than the following, that is, it will penetrate into ſtill ſmaller veſſels, but it is thought to have the inconvenience of being more brittle; ſo that after corroſion, the moſt ſlender of the veſſels are more liable to break down in handling the preparation.

The following is the receipt which the celebrated Meſſrs Hunter of London, have commonly made uſe of. It is leſs brittle and produces a firmer coheſion of parts, with nearly the ſame conſiſtence as the former. Beſides, it enters very ſufficiently into the capillary veſſels.

Second Receipt.

Take of pure roſin eight ounces, of wax four ounces, of Venice turpentine a ſufficient quantity, that is, about

eleven or twelve ounces, to procure a proper conſiſtence to the injection.

The method of aſcertaining the due conſiſtence and the neceſſary firmneſs of the injection, is by taking up any quantity of it, whilſt melted, with a ſmall wooden ſpatula, and then letting it fall drop by drop on the ſurface of cold water. This immediately ſpreads and forms into a thin plate. By rolling it between your thumb and finger firſt moiſtened, or in the palm of one hand with the fingers of the other, both previouſly made wet to prevent ſticking, turn it into the ſhape of a cylinder or ſmall blood-veſſel, then throw it into a baſon of cold water, and let it remain till it is quite cold. If it is then of ſuch a conſiſtence as not to yield to a very ſlight force, when preſſed between your thumb and finger, and yet ſo ſoft as to be capable of bending readily without breaking, it has the due medium of firmneſs and flexibility which is deſired. If it appears to be too ſoft, a further quantity of wax and roſin are to be added in the above mentioned proportion, till it acquires the wiſhed for conſiſtence. If, on the other hand, it is too hard, a proportionably larger quantity of the Venice turpentine is to be added. The ſame precaution is to be obſerved, if we make uſe of roſin, wax and ſpirit of turpentine, as directed in the firſt receipt.

Operators ſeldom are at the trouble of weighing the ingredients; they generally judge of the reſpective weights and proportions of each by the eye. This method of determining them will anſwer very well for perſons who have acquired experience; for the different ſeaſon of the year when the injection is made, and the different conſiſtence or purity of the wax and roſin, with other little circumſtances which ſometimes happen, occaſion ſome little variation. In general there is not ſo great danger of ſpoiling the preparation, by making the compoſition a little ſofter than is required, rather than harder, becauſe it grows ſome what harder by time, and alſo by ſteeping the

the parts injected in water, for the ſake of waſhing off the menſtruum that we have made uſe of for performing the neceſſary corroſion.

Theſe injections are well ſuited to make corroded preparations of the viſcera, as of the heart, lungs, liver and kidneys. Yet I doubt not but perſons of ingenuity, who ſhall be at the pains to render themſelves converſant in the art of injection, by giving attention to every circumſtance, will acquire further ſkill and may find other ſubſtances, equally fit for injection without being ſo liable to become brittle, which thoſe compoſitions I have given above are, in ſome degree, even when made with the utmoſt care and exactneſs.

To know whether any ſubſtance of which a perſon wiſhes to make a trial for injection, will withſtand the action of the menſtruum he means to employ, it is ſufficient to put a piece of the compoſition to be uſed as an injection into a ſmall quantity of the menſtruum, and let it remain in it for a week or fortnight; by that means he can judge of its goodneſs, before he is at the trouble of making, or of ſpoiling an injection, as the caſe may happen.

Of the COLOURS.

The colours we commonly uſe to diſtinguiſh the different orders of veſſels are, 1ſt. Vermilion for colouring the injection for the arteries. 2d. Blue verditure, or what is called Pruſſian blue, for the injection to be thrown into the veins. 3d. For colouring the injections to be thrown into the ureters and pelvis of the kidneys, and the tracheal veſſels of the lungs, what are called in Engliſh king's yellow and flake white are moſtly uſed. A variety of other colours may be employed, but theſe are the principal and the beſt.

We ſhould obſerve to melt the wax thoroughly over a ſlow fire, and the colouring powders ſhould be added by degrees, ſtirring them well in at the ſame time, before the

other ingredients are added. This method prevents any efferveſcence, which often happens when there is too great a fire, or when all the ingredients are mingled together before the colouring powders are added, eſpecially the two laſt. The quantity is judged of by trial.

Of the proper MENSTRUUM.

I now paſs to the third head, viz. to conſider what ſubſtances will anſwer, for conſuming all the parts ſurrounding the injection, and leaving the matter in the veſſels themſelves untouched.

The beſt I have yet tried is the concentrated fuming acid of marine ſalt, which comes over in the diſtillation employed in the proceſs for making glaubers ſalt; it ſhould not be diluted with water, nor be dulcified, otherwiſe it becomes too weak to anſwer the purpoſe, or at leaſt the time required for compleating the corroſion is thereby protracted beyond what is needful.

The concentrated acids of vitriol and of nitre, are no leſs powerful to deſtroy all the animal ſubſtance, ſurrounding the injected veſſels, but the objection to which, from ſeveral trials, they appear to be liable, is that they are ſuppoſed to criſp the veſſels; at leaſt the ſpirit of ſea-ſalt has been moſt uſed, and concluded to be the leaſt exceptionable menſtruum for this operation.

Such were the ſentiments I communicated, in my memoir to the royal academy of ſurgery at Paris in the year 1764, ſince which, Monſ. Süe, royal profeſſor of anatomy in the ſchools of ſurgery, and in the royal academy of painting and ſculpture at Paris, having honoured my communication to the abovementioned academy, with a ſection in his treatiſe entitled Anthropotomie, chap. 2. ſect. 25. from page 70 to page 84; after acknowledging that he received the art of making thoſe preparations from me, thus adds. "Since that time my nephew and I, having worked at them a great deal, and with abundant ſucceſs,

ceſs, we have diſcovered, that we may change the menſtruum, without injuring the preparation at all, and employ aqua fortis, or the nitrous acid in place of the fuming ſpirit of ſalt; and that aqua fortis is even a more perfect menſtrum, than the ſpirit of ſalt, in as much as the colour of the injection is thereby leſs changed, and the ſmall veſſels better preſerved. Beſides the difference of expence is conſiderable, as the ſpirit of ſalt is worth eighteen livres a pint, whilſt the aqua fortis coſts at moſt but two livres, and the effect is the ſame for quantity. I have alſo employed the ſpirit of nitre for the ſame purpoſe, with great ſucceſs*."

In reſpect to the fourth Principle, namely, the Addreſs of the Operator, and wherein it conſiſts.

He ought to guard againſt cutting away or removing the cellular and other ſurrounding parts, before he has made the injection. In fact, theſe give firmneſs to the veſſels, and prevent their ſtretching unnaturally, or aſſuming forms contrary to nature, from the impulſe of the injection when drove into them by the hand of the anatomiſt. Theſe ſubſtances enable them to reſiſt the too great extenſion and yielding to the force applied.

The injecting pipes ought to be proportioned to the ſize of the veſſels through which the injection is to be made.

It is proper to ſoak thoſe parts in warm water, which we are about to inject, for a ſhorter or longer ſpace of time, as well to waſh them clean, as to carry off the blood and other fluids, and the better to diſpoſe thoſe parts to receive the injection, with which they are to be filled.

The

* Monſ. Morgan, Docteur en medicine de la faculté d'Edinbourg, en a donné une deſcription exacte à l'Academie royale de la chirurgie, et c'eſt de lui que je tiens l'art de préparer ces parties; mais depuis après y avoir beaucoup travaillé, mon neveu et moi, et y avoir eu beaucoup de ſuccès, nous avons decouvert qu'on pouvoit changer le menſtrue, ſans préjudicier en rien à la preparation, et employer, au lieu de ſel fumant, l'eau forte, qui eſt même un menſtrue plus parfait, que l'eſprit de ſel, puiſque la couleur de l'injection eſt moins changée, et que les petits vaiſſeaux ſont mieux conſervès. D'ailleurs la différence eſt encor bien grande pour la dépenſe, puiſque l'eſprit de ſel vaut 18 liv la pinte, au lieu que l'eau forte ne coûte tout au plus que 2 liv, et que la quantité eſt la même pour l'effet. J'ai employè auſſi avec beaucoup de ſuccès l'eſprit de nitre. Anthropotomie, pag. 83. 84.

The ſubſtance employed for the injection ought to be entirely melted over a moderate fire, and be heated to the exact degree that will not permit the cooling of it too faſt, upon coming into contact with the parts into which it is impelled, nor ought it to be ſo great, on the other hand, as to burn or criſp the veſſels, or prove troubleſome to the operator in handling the ſyringe or pipes, whilſt he is making the injection.

The injection ſhould be thrown into the part to be prepared, at one uniform impulſe, made ſlowly and evenly, with a ſteady hand, and with ſo little force as not to endanger a rupture of the veſſels in the ſoft parts. When the injection is finiſhed, the pipes ſhould be corked or otherwiſe cloſed, and the parts injected ſhould be ſuffered to cool by degrees. If they are plunged at once into cold water, before the ſubſtance of them has acquired a certain degree of hardneſs and firmneſs, a contraction in the elaſtic coats of the veſſels may be produced, ſufficient to occaſion a rupture, eſpecially in the capillaries, which will always be followed by an extravaſation of the injected ſubſtance.

Having expoſed the injected parts to the air during an hour or two, it is proper to commit them to cold water all night, to cool and to harden them thoroughly. After taking them out of the containing veſſel and abſorbing the water from their ſurface with a ſponge gently applied, or ſuffering it to run off by draining, they ſhould be put into a ſufficient quantity of the menſtruum to cover the preparation entirely.

The next conſideration is how to make uſe of the menſtruum for corroding the parts to be diſſolved and removed from the veſſels. For this purpoſe the operator ſhould be furniſhed with a china bowl or a ſtone veſſel, on which the menſtruum can make no impreſſion; or what will anſwer ſtill better, a glaſs veſſel with a mouth ſufficiently large to put in and take out the injected parts, without any difficulty.

difficulty. It would be well to furniſh it with a proper cover to reſtrain the acid fumes from eſcaping. I have always uſed a cover of cork lined with wax, into which, whilſt it is in a melted ſtate, the inferior ſide of the cover may be dipped; and this cover muſt be cut ſo as to fit exactly the mouth of the veſſel. The great advantage of a glaſs veſſel over the others is its tranſparency, whereby we are able to ſee how the corroſion goes on, and to judge when it is finiſhed. This takes up from ſix or ſeven days to a fortnight or three weeks, according to the nature of the part to be prepared, and to the quantity and concentricity of the menſtruum employed, in which it ought to be entirely covered. When the acid is very dilute, it proves rather antiſeptic and a preſerver of animal ſubſtances, than a corroſive menſtruum.

Of diſengaging the Corroded Subſtance.

Fifthly. The laſt part of the operation conſiſts in diſengaging the looſened and corroded ſubſtance from that of the injection. In this piece of buſineſs we ought to take the greateſt care, if we wiſh to avoid breaking down the beautiful ſmall veſſels of the part. With this view the acid ſpirit employed as a menſtruum ſhould be decanted from the injection with great caution, whenever the corroſion of the ſurrounding ſubſtance is complete, ſo that it no longer adheres to the veſſels. In place of the corroding menſtruum, ſoak the preparation in ſimple water for three or four days. The looſe ſubſtance may be removed from the veſſels, by pouring freſh water over the preparation ſlowly, and in ſmall quantity at a time; or otherwiſe we may put the preparation in a veſſel pierced with holes, like a cullender, and place this in ſuch a manner as to receive a gentle current or ſtream of water. If we ſhould place the veſſel near the noſel of a pump, and under the droppings or ſmalleſt ſtream which we can procure to fall from it, the preparation may be thus cleanſed from the looſe corroded matter with which it is encompaſſed.

But

But the method I have diſcovered, and always practiſed as the ſafeſt and beſt, is to make uſe of a ſmall ſyringe, the piſton whereof works eaſily, with which, whilſt the preparation is covered three or four inches over with water, I ſyringe gently, ſo as to waſh and clean it entirely from the corroded ſubſtance which is but looſely attached to it. In this manner, uſing proper care, it may be performed perfectly without breaking down any, even the fineſt parts, of the tender veſſels.

But if more force than needful is employed, even the droppings of a pump from a too great height, when the preparation is taken out of the water, will ſometimes break down the extremities of the ſmall veſſels, and mar the beauty of it..

After all theſe directions have been well executed, the preparation is to be ſuſpended for ſome time in a ſafe place, till it is dry; then it is to be fixed on a wooden pedeſtal, having a ſocket like a candleſtick, in which it may be faſtened with a little glue or melted wax. Then let it be covered with a tranſparent glaſs in form of a globe or bell, with the mouth downward, to guard it from accidents. This finiſhes the work.

Theſe preparations give us a moſt exact knowledge of all the ramifications and anaſtomoſes of the veſſels, and often of the junction of the arterial and venal ſyſtem, when the injection is fine enough, and ſo ſucceſsfully thrown from the arterial trunk as to penetrate into the veins and fill that ſyſtem of veſſels in the organ prepared, completely, at one and the ſame time. Thus I have filled both the emulgent arterial and venal ſyſtem of veſſels, in a kidney, at one coup de main, through a ſingle pipe fixed in the great trunk of the emulgent artery, the correſponding trunk of the vein being ſhut up with a ligature. But it is more common for the injection made by the artery to ſtop at the extremities of the evaneſcent branches, and to fill the venal ſyſtem by a ſecond injection, drove through the great trunk of the emulgent vein.

Upon

Upon the whole, theſe teach us, in the beſt manner poſſible, the true and intimate ſtructure of the viſcera in general, and of every particular part; from whence we may aſſert, without fear of any juſt cenſure, that preparations thus executed are exceedingly uſeful, and enrich the cabinet with choice and beautiful ſpecimens of anatomy.

I have only to add that, in order to enable the artiſt to ſucceed and puſh his diſcoveries, it behoves him to learn the art of preparing the ſubject by diſſection. This is a new branch, though intimately connected with the foregoing: There are few books that teach it; but one very excellent treatiſe on the ſubject is publiſhed in French by Monſ. Süe, already quoted by the title of anthropotomy, or the art of diſſecting, injecting, embalming and preſerving the parts of the human body; which, as I think it will greatly contribute to improve anatomy, I have ſome thoughts, at my leiſure, to tranſlate into Engliſh, for the benefit of the ſtudents of anatomy, phyſic and ſurgery in America.

I here ſubjoin the manner of making Wax Preparations by Monſ. Süe.

"WHEN we have a mind to make any preparation of wax, we ought to begin by moulding the part we wiſh to imitate with freſh plaiſter of Paris made very fine, taking care to oil it previous to the application of the plaiſter.

"When the plaiſter laid on the ſurface of the part is cold, remove all the pieces that compoſe the mould one after another, taking care not to break any of them.

MANIERE DE FAIRE LES PREPARATIONS EN CIRE.

LORSQU'ON veut faire quelque preparation en cire, il faut commencer par mouler la partie que l'on veut immiter avec du platre frais et bien fin, ayant l'attention de bien huillér la partie avant que d'appliquer le platre. Lorſque le platre ſera refroidi de deſſus la partie, alors on otera toutes les pieces qui compoſent le moule, l'une après l'autre, prenant garde qu'aucune

After leaving them to dry for ſome time, they may be ſafely uſed. For this purpoſe, melt a ſufficient quantity of virgin wax in a ſkillet, over a gentle fire, and colour it according to the colour of the part which is to be imitated, with carmine, or other paint; or if the piece to be imitated has ſeveral parts of different colours, we muſt not then colour the whole piece at once, but are to place the colour required upon each part, after the piece has been melted.

" Whilſt the wax melts, prepare the mould, which we muſt be careful to oil well with a little bruſh to prevent ſticking; then join all the pieces of the mould together, which muſt be tied faſt with ſmall cords or twine; and that the air may not paſs through the cracks or joinings of the pieces of the mould, place ſome clay on the outſide, by way of luting.

" The mould being thus prepared, and having taken care to leave an opening, we pour the wax into the mould through it, and then carefully turn the mould every way, in order that the wax may ſpread equally through all the interior parts, of it until it is cold. If it be found that the piece is not thick enough, we are to pour on more melted wax, and turn it as before; then let the piece cool in the mould: afterwards, with proper precaution, we are to take the pieces of the mould apart, one after the other. The preparation being taken out of the mould entire, we muſt take off the ſuperfluous portions of wax which penetrated the

qu'aucune ne caſſe. On le laiſſe ſecher pendant quelque tems. Enſuite on peut s'en ſervir. Pour cet effet on fait fondre de la cire vierge dans un poulon à petit feu, et on le colore, ſuivant la couleur de la partie qu'on veut repreſenter, avec du carmin, ou quelque autre couleur; ou bien, ſi la partie a pluſieurs parties de couleur differente, alors on ne colore point la piece. Et on met la couleur ſur chaque partie après que la piece a ete fondüe. Pendant que la cire fonde on prepare le moule, qu'on a le ſoin de bien huiller avec un pinceau. Enſuite on reſemble toutes les pieces du moule, qu'on retient unies avec des cordes ou de la ficelle, et pour que la cire ne paſſe par les fentes ou les jointures des pieces du moule, on y met ſur les jointures exterieurement, de la terre glaiſſe. Le moule preparè ainſi, et ayant eu l'attention de y laiſſer une ouverture, on verſe la cire dans le moule, et l'on à le ſoin de tourner le moule en tout ſens, pour que la cire ſe repande egalement par tout l'interieur du moule, juſques a ce qu'elle ſoit rafroidie. Si vous jugez que la piece ne ſoit pas aſſez epaiſſe, vous remettez de nouveau de la cire, et vous faites comme ci deſſus. Aprés quoi on laiſſe rafroider la piece dans le moule, et enſuite on tire avec beaucoup de precaution les pieces du moule, l'une après l'autre. La piece etant entierement depouillée, on la repare. C'eſt a dire qu'on ote de deſſus.

the joinings of the pieces of the mould, which being entirely repaired, we then colour the different parts which compoſe the piece with colours ſuitable to each part, that the arteries may be coloured with vermillion; the veins with Pruſſian blue; the muſcles with carmine; and ſo of the other parts; *which finiſhes the preparation.*

"*Note*, When we wiſh to make the wax leſs brittle, we muſt add ſome ſpermaceti to it; and ſometimes a little of the fineſt Venice turpentine that can be procured."

ſus les portions èxcedentes de cire qui ſe ſont gliſsées entre les jointures des pieces du moule. La piece etant entierement reparèe, on colore les differentes parties qui compoſent la piece avec des couleurs convenables a chaque partie, en ſorte que les arteres ſeront colorées avec du vermillon; et les veines avec du bleu de Pruſſe; les muſcles avec du carmin; ainſi des autres.

Nota que quand on veut rendre la cire moins caſſante on y adjoute le blanc de baleine, quelque fois auſſi un peu de terebentine de Veniſe, tout ce qu'il y a de plus fin.

N° XLIII.

Of a living Snake in a living Horſe's Eye, and of other unuſual Productions of Animals. By JOHN MORGAN, *M. D. F. R. S. London, Profeſſor of the Theory and Practice of Phyſic, Philadelphia.*

Read June 5, 1782.

WHETHER there is ſuch a thing in nature as equivocal generation, by which is to be underſtood the production of any new animal independant of a parent ſtock of the ſame kind, has been a ſubject of controverſy amongſt philoſophers; ſome aſſerting the reality of this doctrine, whilſt others, as the celebrated Harvey and his followers as ſtrenuouſly reject it. The latter, which is now deemed the orthodox ſide of the queſtion, affirm that the young of all are produced from an egg, furniſhed by the female, and fœcundated by a male animal. From the light thrown upon this ſubject, by the deep reſearches of Hippocrates, Galen and Ariſtotle,

among the antients; and amongſt the moderns by Malphigi, De Graaf, and above all others by the beforementioned renowned Harvey, phyſician to king Charles the firſt of England, (the diſcoverer of the true circulation of the blood) this induction ſeems to be eſtabliſhed upon an induction of facts and experiments, carrying with them the force of conviction, ſo far as that induction reaches.

The only room which ſome ſuppoſe there may be for doubt of the univerſality of the propoſition is, that a variety of animals have been found, at different times, to exiſt in the bodies of other animals, and in extraordinary places, which neither the diſcoverers of thoſe animals, nor others have been able to trace, with clearneſs and certainty, to what mankind, in general, can deem a probable or ſatisfactory origin. Whether it be owing to an impatience to arrive at ſome concluſion, which can ill brook the difficulties of the inquiry, or to the rareneſs of the caſes falling under the notice of perſons capable of making a thorough inveſtigation, and the tedious progreſs of experimental knowledge; or whether it is that we are apt to ſuppoſe the ſubject does not admit of mathematical certainty from the light of philoſophy, I know not; but ſome men have had recourſe to the doctrine of equivocal generation, to account for thoſe productions, as Ariſtotle and his followers had, in other caſes, to certain occult qualities; a term by which they have endeavoured to conceal their ignorance of what they could not explain, but were unwilling to confeſs. Nor are there wanting, in the preſent day, many perſons, who will ſooner deny the teſtimony of their ſenſes, than allow the exiſtence of an animal production, which they know not how to account for.

This I ſuppoſe to proceed from a falſe pride, or an apprehenſion of being deemed credulous in a philoſophic and enlightened age; and becauſe, in times of ignorance, the paſſions of illiterate men were wrought upon by fictions to believe in prodigies, whereby they were led blindfold, into opinions

opinions of religion and philoſophy, which had no ſolid foundation, the race of ſceptics I refer to deem it manly not only to with-hold their aſſent from truths they do not underſtand, but to diſown and diſpute the reality of them. They do not conſider that, by ſuch conduct, they endeavour to diveſt themſelves and others of their rational faculties, and of that natural curioſity implanted in man by his Creator, for the wiſeſt purpoſe, as a guide for inveſtigating facts, in order to lead him to knowledge, which has given birth to diſcoveries of the greateſt importance to mankind.

In anſwer to the cavils of minute philoſophers, I would briefly remark the firſt ſtep to new diſcoveries, is an exact attention to the phænomena of nature, unbiaſed by preconceived hypotheſes, and that it is as much a mark of a defective underſtanding to admit too little for truth, upon evidence, as to believe too much from credulity.

I have been led into the above train of obſervations from a ſingular phænomenon that may be now ſeen in this city, and which is worthy the inſpection of the curious. It is advertiſed in the public newſpapers, viz. the Pennſylvania Gazette, May 23d, as worthy of the attention and critical inſpection of all curious perſons, whether philoſophers or phyſicians, and particularly the latter, as it may, for what they know, if properly examined into, throw ſome uſeful light upon the functions and diſeaſes of the animal body.

What I refer to is an horſe with a ſnake in its eye, to be ſeen in Arch-ſtreet, between Sixth and Seventh ſtreets, not only poſſeſſed of mere life, but endowed with a very briſk locomotive faculty. True philoſophers will not treat the aſſertion as idle, fictitious or romantic, but ſee and judge for themſelves.

The writer of this piece has undertaken the preſent taſk, on purpoſe to excite every claſs of people to ſatisfy themſelves of the reality of the fact, that when recorded in the very place where all have it in their power to determine its exiſtence, on the teſtimony of their own eyeſight, they may not

not plead ignorance and unbelief. He profeſſes, for his own part, to be as little credulous or liable to impoſitions, from accounts of pretended miraculous appearances, as his neighbours, however learned. Indeed he has ever ſtrenuouſly oppoſed, and thinks he ever ſhall, what he deems empty tales of viſionary ſpeculatiſts, bred by weak fancies, or raiſed by deſigning men, to amuſe or deceive the vulgar; but he admires and reveres the unſearchable wiſdom of the divine architect, who framed this ſpacious univerſe, teeming with myriads of animal beings, as well in thoſe inſtances where his deſign and footſteps are viſible, as in thoſe which lay more remote from human comprehenſion. Upon the firſt relation of this curious hiſtory from others, unacquainted with the ſtructure of the eye, and therefore more likely to paſs a wrong judgment; and, till he had an opportunity to examine it himſelf, he believed the appearance to be ſome unuſual diſeaſe, or a filimentary production on the cryſtalline humour, from a ſtroke or inflammation of the eye, and that a convulſion in the nerves of its coat might produce an irritation in that organ, and a tremulous motion, which might impoſe upon thoſe who, not knowing how to account for the appearance, ſhould content themſelves with calling it a ſnake in the eye, merely from its reſemblance, on firſt ſight, to that animal. But from the cloſeſt ocular examination, with unwearied attention, repeated more than once, he conceives he is not miſtaken, in aſſerting that there is a real ſnake in the eye; which, from the vivacity and briſkneſs of its motion, exceeds that of any worm, and equals that of any kind of ſerpent he has ever ſeen.

To ſatisfy the public in general, as well thoſe who have now an opportunity of ſeeing it, as ſuch who may happen never to ſee it, I think it will not be amiſs to deſcribe its appearance, and to deliver what I have been able to collect of its hiſtory.

The

The horſe in whoſe left eye this extraordinary *luſus naturæ* is viſible, is of a ſorrel colour, nine years old; it belonged to Doctor Dayton near the lines at Elizabeth-town, and, I am told, appeared to have no uncommon appearance in either eye, till within a few months ago. The firſt particular circumſtance which excited the owner's attention was, that having lent him to a friend to take a ride in a chair, although it was not known to be vicious or unruly before, it could not now be kept under any government, but ran away with, and daſhed the chair to pieces. The right eye ſtill continues in a ſound ſtate.

Soon after, viz. about ten weeks ago, Mr. Richard Wells, merchant of this city, a gentleman of probity and of great philoſophic knowledge, being at Elizabeth-town in company with Doctor Dayton, this gentleman told him he would ſhew him a curioſity as great perhaps as he had ever ſeen, namely, a living ſnake in a living horſe's eye. Mr. Wells then deſiring to ſee it, upon looking into the eye, diſcovered the animal very plainly, in a conſtant ſerpentine motion, but neceſſarily in a ſomewhat convoluted form, as its length was equal, as nearly as he could judge, to two diameters and an half of the eye, which could not meaſure leſs than between three and four inches. The head and tail, or if you pleaſe, the two extremities of the animal were then viſible, and the horſe's eye ſtill retained its tranſparency enough to admit ſeeing the whole of the ſnake diſtinctly.

The horſe was ſoon after purchaſed by a free negro, on purpoſe to bring to Philadelphia for ſhow, in order to gratify the curioſity of the virtuoſi of every claſs, by giving them an opportunity of ſeeing and contemplating ſo curious a phænomenon, and of communicating the reſult of their inquiries to the learned, for the information of the public at large.

At preſent, apparently from the briſk and almoſt conſtant motion of the animal, which is ſomewhat increaſed in

in length, ſince the inſpection at Elizabeth-town, and which is as thick as a knitting needle, or piece of common twine, as nearly as can be determined through the intervening medium, the aqueous and vitreous humours of the eye are confounded (the fine cellular texture of the latter being broke down) and tinged with the ſofteſt part of the cryſtalline, ſo as to aſſume ſomewhat of a white milky appearance, bordering on the colour of a cataract. The Iris appears to be greatly dilated, or rather wholly deſtroyed. For the ſeptum, or partition which ſeparates the anterior from the poſterior chambers, in a ſound eye, muſt be broken down, as the animal, or, to ſpeak like a ſceptic, the animal appearance of a ſnake is continually receding into the fundus and back part, and by times coming forward into the anterior part of the eye, with a convoluted briſk motion. I cannot think a ſnake of the ſame ſize moving briſkly in a tumbler-ful of fair water, or of water diſcoloured with a tea-ſpoonful of milk, would be more viſible; but the coats of the eye and humours have now ſomewhat of a milky appearance, or colour of an incipient cataract.

It may be juſtly preſumed, that whatever might be the ſtate of viſion, at firſt appearance of this ſurpriſing phænomenon, that eye muſt be now blind. The lids are commonly cloſed, probably owing to pain excited in the eye by ſo troubleſome a gueſt; but there is no bloodſhot appearance on the cornea, though the ſurrounding parts, namely, the palpebræ, are a little tumid. To get a view of the eye, the keeper commonly ſtrikes the horſe on its back with an open hand, at which, as if frightened, it opens the lid of the left, as well as widens the opening of the right eye, which continues diſcloſed but a ſhort time; however this gives an opportunity for inſpection for five or ſix ſeconds of time together, and the blows muſt be repeated to keep the eye open, when a perſon wiſhes to have a longer time for inſpection.

The

The milky appearance has for ſome weeks grown gradually more opaque; from which circumſtance it is probable the diſeaſe occaſioned by the preſence of an extraneous body, or unnatural animal irritating the organ, will gradually produce too great obſcurity to afford that ſatisfaction in viewing it, which hitherto it has done and ſtill continues to afford.

It has been my wiſh, and I have expreſſed my opinion to ſeveral gentlemen that it would be worth while, to make up a ſum of money and purchaſe the horſe for ſake of diſſecting the eye, whilſt the animal is yet alive, but no notice has been yet taken of it: Perhaps the owner keeping it for ſhow places too high a value upon it. I have further deſired, if that purchaſe is not made, to have an opportunity of taking out the eye and diſſecting it immediately after death, whenever that event takes place, if it happens where I am.

The eye has been inſpected by ſeveral gentlemen of the faculty, who are aſtoniſhed, and at a loſs to account for the appearance on common principles or from known diſeaſes; a queſtion then naturally ariſes in the minds of moſt who have ſeen or heard of it, viz. If it be a real ſnake or other living animal, how it got there, or whether there are other inconteſtible hiſtories to match it, in the annals of medical hiſtory, of animals bred in man or other animals, as difficult to be accounted for?

I anſwer, Facts are what I am more concerned to eſtabliſh than ſpeculative opinions; therefore inſtead of leading to theories that may be idle and groundleſs, I ſhall be ſatisfied to refer all who doubt the reality of its being a ſnake in the eye, firſt to the hiſtory of the Guinea worm, of which I have had more than one caſe falling under my own care, and have ſeen others in the Pennſylvania hoſpital, extracted from the leg, ſeveral yards in length; ſecondly, to the well known hiſtory of a jointed worm bred in the liver of Mrs Holt, in this city, about

thirty years ago, of about twenty inches long and near three in circumference, recorded in the medical eſſays of a ſociety of phyſicians in London. This worm I have ſeen ten years after preſerved in ſpirit, in the anatomical cabinet of the celebrated Dr. William Hunter of that place; and thirdly, I refer to the hiſtory and engraving of one exactly ſimilar, as large as the life, inſerted in the ſecond volume of Edinburgh medical eſſays, plate fourth; and laſtly, to autopſy, by examining the eye of the horſe in queſtion, which will afford ocular demonſtration of the fact.

I ſhall add to theſe ſome obſervations of that prince of anatomiſts in his day, the famous Ruyſch, who, as Dr. Haller atteſts, from a practice of diſſection continued for near eighty years, with a diligence, ſkill and accuracy in examining into morbid bodies, and the niceneſs of his diſſections and of his anatomical injections, exceeded all his cotemporaries; and in fine, whoſe teſtimony in thoſe matters was looked upon by Boerhaave and Haller, and by every medical writer ſince, to be as inconteſtible authority as that of any other perſon whatſoever.

In his firſt volume, obſervation the 16th, he ſays, "daily experience proves that worms may be generated in all parts of the body. I ſtrangled a dog that was very lively three hours after being fed, with a view to examine the lacteal or milk veſſels. On opening the belly of it a live worm, at leaſt two ſpans in length, ſkipped out. I could diſcover nothing amiſs in the omentum, nor any ſolution of continuity of the parts; and both the meſentery and inteſtines were ſound."

Again he ſays, obſervation 54, "I have had room to doubt whether, as Harvey and his followers affirm, all animals are produced out of an egg, from worms being found in the arteries of living horſes; as alſo from worms ſeen in the parenchyma, or the glandular ſubſtance of the liver, as it is now called, and alſo in the cyſtic duct and biliary pores of ſheep, and very often in the gall-bladder.

I remember

I remember once to have ſeen them in the human kidneys, and ſuch as are more frequently met with in the kidneys of dogs. That worms have been ſometimes found in the brain, no body can deny who will be at the trouble of turning over the writings of authors of high repute."

By what paſſages thoſe animals or their eggs were inſinuated into the interior parts of the body, is not eaſy to determine. It does not ſeem probable that they could reach the forementioned places through the pores of the ſkin or the organs of reſpiration; much leſs that their eggs were taken in at the mouth, and from thence proceeded to their reſpective places; nor yet is it likely they could remain entire in the ſtomach, where, in the proceſs of chylification, there is a remarkable fermentation and breaking down the parts of the food. Nor can the chyliferous or lacteal veſſels afford a paſſage to the eggs in their rout; and laſtly, no one alledges that he has ever ſeen exactly ſimilar worms out of the body.

Were I ſo diſpoſed, or did the deſign of this paper require it, I could to thoſe obſervations add many extraordinary inſtances, of ſtrange, rare, and ſurprizing productions of animals in the human body, from the works of the celebrated Bartholine, phyſician to the king of Denmark, and from other grave and learned authors of unexceptionable repute. But here I pauſe, to reverence the hand that framed not only our bodies, but thoſe of the meaneſt reptiles, with an exuberance of ſkill, which proclaims that they are not the effect of chance; and acknowledging I am loſt in wonder, I leave the fuller explanation of the uncommon productions, to ſome happy genius that may ariſe, if ever it ſhould pleaſe God to produce ſuch an one into the world, who by tracing out the footſteps of the Creator, ſhall be able to throw clearer lights than we yet have, upon theſe abſtruſe ſubjects.

N° XLIV.

Some Account of a motley coloured, or pye Negro Girl and Mulatto Boy, exhibited before the Society in the Month of May, 1784, *for their examination, by* Dr. JOHN MORGAN, *from the Hiſtory given of them by their owner Monſ. Le Vallois, Dentiſt of the King of France at Guadaloupe in the Weſt Indies, as follows.*

ADELAIDE, the little girl now before the ſociety, is aged two years and little more than one month, is of a clear black colour, verging to brown, except that ſhe has a white ſpot bearing ſome reſemblance to an aigrette; the point of which is at the root of the noſe, and it riſes into the hair, above the forehead, of which it occupies above an inch in width, from the margin to the fontenelle. In this part the colour of the hair is white, and it is curly like the hair of negroes in general, and thicker in that part than on any other part of its head. In the middle of its forehead and on the aigrette, is a large black ſpot; on the external ſide next to the temples, about one half of each eye-lid, both upper and under, is black, and the remaining half next to the noſe is white.

The eyes are black and lively, a little to the left and towards the middle of the chin a white ſpot begins, which is long in proportion to its breadth, but of leſs magnitude than that of the forehead: It ſtretches under the chin to the upper part of the throat. The neck, the upper and under part of the cheſt, the ſhoulders, the back, loins and buttocks to the junction with the thighs, and the pudendum, are of the colour of her face, but the loins and the thicker part of the buttocks are of a deeper black.

The

The arms from the upper and middle part are white, and interſperſed with black ſpots. There are ſome ſmaller and more numerous about her knees than elſewhere.

Upon the large black ſpots there are alſo many ſmaller and blacker which are very glaring. Many of theſe ſpots divide into four, five and ſix rays, reſembling a ſtar, which are not obſerved but by a cloſe inſpection, and then they are very viſible. In ſeveral parts thoſe ſpots, being of different ſhades, give an exact picture of lunar eclipſes, as they are commonly repreſented in the books of aſtronomy. The hands, the middle part of the fore arms, the inferior and middle parts of the legs and feet are black, which have a pretty ſtriking reſemblance to gloves and to buſkins.

The white that prevails over the breaſt, and over the belly, arms and thighs, has a lively appearance. The ſkin is ſoft, ſmooth and ſleek.

Adelaide has fine features; we meet with few negroes of ſo beautiful a form. In her temper ſhe is cheerful, gay and ſportful, and as tall as children of her age generally are, and hath evidently a very delicate temperament, yet enjoys pretty good health, neither hath ſhe eyes, nor ears, nor any particularity in her features, or external conformation, like what may be ſeen at the firſt inſpection in thoſe who are called white negroes, whoſe ſkin is altogether of a dead white colour, and whoſe woolly white hair and features reſemble thoſe of their negro parents.

From this detail we may remark that the alteration of the natural colour of Adelaide, takes place over the ſame parts of the body, for the moſt part, as over the body of Maria Sabina, of whom Monſ. Buffon gives an account; and conſidering it as a well authenticated fact, from all the information that has been received of Adelaide, that ſhe had a negro father and negro mother, we are led to believe, that the Engliſh account under the portrait of Maria Sabina is exact, and not aſſerted merely for the

ſake

ſake of covering the honor of the mother, and of the ſociety in which ſhe was a ſlave.

The pyed mulatto boy is named Jean Pierre. He is a month younger than Adelaide; but from his figure, which is robuſt, he appears to be ſix months older. He as well as Adelaide both belong to Monſ. le Vallois. He was born at Grandterre, Guadaloupe, of a negro wench named Carolina and of a white man, an European, whoſe name I did not learn.

A certificate which Monſ. le Vallois has with him, legally authenticated by Monſ. Blin, lieutenant judge, given from under the hand of Monſ. des Effart, king's phyſician, and of Monſ. Cumin, king's ſurgeon, at Grandterre, Guadaloupe, atteſts that Adelaide was born at Gros-Iſlet in St. Lucia, that Bridget her mother is a negro of the Ibo nation, and now reckoned to be about twenty-five years old, and that her father, whoſe name is Raphael, is a negro of the Mina nation. In this certificate it is farther declared that the father of Jean Pierre has white ſpots (that is of a deeper white than his natural ſkin) of the ſame ſhape and in the ſame parts of the body as the ſon, and that the mother and one of the brothers of this boy's European father have like white ſpots, and in the ſame parts of the body.

However it may be in reſpect to thoſe obſervations concerning the ſuppoſed reſemblance of the white ſpots they may bear about them, to thoſe which mark Jean Pierre, it ſuffices to take notice here, that his body is entirely of the colour of a mulatto, except that he has from nature a white aigrette in his forehead like that of Adelaide. The hair in that part is white mixed with black, which is not ſo in Adelaide. The ſtomach, and the legs from two inches above the ancles to the middle of the calf of the legs are entirely of a beautiful lively white; there is alſo a white ſpot in the upper part of the penis. Over the white parts of

of the legs there is a light white down, longer and thicker than children commonly have at this age.

Such is the natural hiſtory of thoſe two extraordinary children; but what cauſes have produced thoſe ſurpriſing phænomena and alteration of the natural colour of their ſkin, are left for others to inveſtigate and explain.

Monſ. le Vallois relates that the mother of Adelaide, whilſt pregnant with her, was delighted in laying out all night in the open air, and contemplating the ſtars and planets, and that the great grandmother of Jean Pierre (a white lady) during the time of her being with child of her daughter, his grandmother by the father's ſide, was frightened on having ſome milk ſpilled upon her. Whether this will account for her daughter and grandchildren being marked in the manner related, and for the ſpots obſerved on the mulatto boy deſcending to him; or whether the ſtrong impreſſion made upon the mother of Adelaide, by the nightly view of the ſtars and planetary ſyſtem, may be conſidered as the cauſe of the very extraordinary appearances in that girl, every one will determine for themſelves; there being many who diſpute childrens being ever marked by the fears, longings, or impreſſions made by mothers on the bodies of their children, at a certain time of pregnancy; for which they endeavour to account in different ways; whilſt others who have known a variety of children born with different marks on them, (which have fallen under their particular notice) are equally confident of thoſe marks proceeding from the cauſes alledged.

Extract

N° XLV.

Extract of a Letter from BERNARD ROMANS, *of Penſacola, dated Auguſt* 20, 1773.

THE common mariners compaſs has always appeared to accurate obſervers as an imperfect inſtrument, but in nothing has it proved to be more defective than in its uſe in ſtorms, the heavieſt braſs compaſſes now in uſe are by no means to be relied on in a hollow or high ſea. This is owing to the box hanging in two braſs rings confining it to only two motions, both vertical, and at right angles with each other, by which confinement of the box upon any ſuccuſſion, more eſpecially ſudden ones, the card is always put into too much agitation, and before it can well recover itſelf, another jerk again prevents its pointing to the pole, nor is it an extraordinary thing to ſee the card unſhipped by the violence of the ſhip's pitching.

All theſe inconveniencies are remedied to the full by giving the box a vertical motion at every degree and minute of the circle, and to compound theſe motions with a horizontal one, of the box, as well as of the card. By this unconfined diſpoſition of the box the effects of the jerks on the card are avoided, and it will always very ſteadily point to the pole. Experience has taught me, that the card not only is not in the ſmalleſt degree affected by the hollow ſea, but even in all the violent ſhocks and whirlings the box can receive, the card lies as ſtill as if in a room unaffected by the leaſt motion.

Lately a compaſs was invented and made in Holland, which has all theſe motions. It is of the ſize of the common braſs compaſſes, the bottom of the braſs box inſtead of being like a bowl, muſt be raiſed into a hollow cone, like

like the bottom of a common glaſs bottle; the vertex of the cone muſt be raiſed ſo high as to leave but one inch between the card and the glaſs; the box muſt be of the ordinary depth, and a quantity of lead muſt be poured in the bottom of the box round the baſe of the cone, this ſecures it on the ſtyle whereon it traverſes.

This ſtyle is firmly fixed in the center of a ſquare wooden box, like the common compaſs, except that it requires a thicker bottom. The ſtyle muſt be of braſs about ſix inches long, round and of the thickneſs of one-third of an inch, its head blunt, like the head of a ſewing thimble but of a good poliſh; the ſtyle muſt ſtand perpendicular, the inner vertex of the cone muſt alſo be well poliſhed; the vertical part of the cone ought to be thick enough to admit of a well poliſhed cavity ſufficient to admit a ſhort ſtyle proceeding from the center of the card whereon it traverſes. The compaſs I ſaw was ſo conſtructed, but I ſee no reaſon why the ſtyle might not proceed from the center of the vertex of the cone, and ſo be received by the card the common way. The needle muſt be a magnetic bar blunt at each end; the glaſs and cover is put on in the common way.

A compaſs of this kind was given by the captain of a Dutch man of war to Capt. Burnaby of the Zephyr ſloop; this gentleman gave it to me to examine, and was very profuſe in his encomiums thereon, ſaying that in a very hard gale, which laſted ſome days, there was not a compaſs but it of any ſervice at all. Indeed to me it appears to deſerve all the praiſe he gave it. My ſtay is ſo ſhort here, as not to allow me time to have one made; but I intend to have one made for my own uſe, and ſhall offer it to the ſociety for inſpection. I hope that this uſeful inſtrument may become univerſal, as navigation certainly will be rendered more ſafe through its means; and I ſhall think myſelf highly honoured, if through the channel of this ſociety it becomes public.

Preſents made to the American Philoſophical Society, ſince its Revival and Incorporation in 1780, *with the Names of the Donors.*

DONORS.	PRESENTS.
1780, Apr. 7. Dr. *Coſte*,	An analytic, etymologic, and argumentative treatiſe on the accent and pronunciation of the Engliſh language.
Dec. 15. Monſ. *Chatilaux*,	His works de la felicité publique, 2 vol.
1781, Jan. 19. Monſ. de *Marbois*,	A pamphlet in French, containing an account of, and propoſals for printing a work of great merit, entitled voyages metallurgiques, &c. Par M. Jars.
Feb. 16. Gen. *Sullivan*,	Specimens of talc, from a large body of this foſſil, lately diſcovered in New-Hampſhire.
Mar. 16. *Sam. Adams*, Eſq.	Copy of a Philoſophical diſcourſe delivered before the academy of arts and ſciences at Boſton, by their preſident James Bowdoin, Eſq.
1783, Feb. 16. Aſſembly of the State of *Pennſylvania*,	One hundred and fifty pounds.
May 2. Mr. *J. M'Henry*,	A curious ſpecimen of petrified pine.

Sept.

1783,	Donors.	Presents.
Sept. 26.	Dr. *Franklin*,	Natural hiſtory of the South of France, by Monſ. L'Abbé Soulavie, 2 vols.
Oct. 17.	Mr. *John Hyacinth de Magellan*,	A collection of Philoſophical tracts, &c.
	Mr. *Godfrey*,	A collection of prints, repreſenting ſome of the principal events during the late revolution in America.
	Abbé de *Fontana*,	Four pamphlets on Philoſophical ſubjects, written by himſelf.
Dec. 5.	Mr. *Van Berckel*,	Six volumes of the Tranſactions of the Batavian Philoſophical Society of Rotterdam.
	Monſ. de *Etienne*,	On a cement impenetrable by water.
1784, Jan. 16.	Mr. *John Felſted*,	A collection of ſpecimens of the ſeveral woods growing in the iſland of Jamaica.
Febru. 3.	Mr. *Warder*,	A collection of animal calculi.
	Mr. *Mandrillon*,	Voyageur Americain. An anſwer to the queſtion propoſed by the Academy of Sciences at Lyons, viz. Whether the diſcovery of America has been advantageous to mankind or not? both written by himſelf.

Feb.

1784. Donors.	Presents.
Feb. 20. *Samuel Vaughan*, Eſquire,	A $3\frac{1}{2}$ feet acromatic teleſcope, completely mounted on a mahogany ſtand, with rack work to move it along the meridian, or parallel to the equator, made by Mr. Dolland.
	A Gunter's ſcale improved, for navigation and aſtronomy, by Mr. John Robertſon, with a deſcription thereof by Mr. Mountaine.
	A magazine microſcope, conſiſting of a ſingle, double, and ſolar microſcope.
Mr. *William Parker* by Mr. *S. Vaughan*,	A burning lens $11\frac{1}{2}$ inches, with a ſecond lens of $6\frac{1}{2}$ inches diameter, completely framed and mounted on a mahogany turned cone, with rack work, pillar, pinnion and apparatus for placing the whole in a proper ſituation.
March 19. Dr. *Franklin*,	A treatiſe in French, on the ſubject of air balloons.
Samuel Vaughan, Eſq.	A copy in manuſcript of Dr. Prieſtley's experiments on phlogiſton, and the ſeeming converſion of water into air.
Apr. 2. Mr. *J. Vaughan*,	Dr. Prieſtley's works, complete in 33 volumes.
	Dr. Prieſtley's biographical and hiſtorical charts.

April

1784. Donors.	Presents.
April 9. *Samuel Vaughan*, Esq.	Dr. Price on reversionary payments, annuities, &c. 2 vols. Transactions of the society of arts, &c. 1783, 1784. Tracts published by the society for constitutional information, 1783. Proceedings at Quebec, 1773. Lind on diseases incident to hot climates. Plinii secundi hist. nat. Antiquitates Romanæ. Rowe's fluxions. Gregory's practical geometry. Hamilton on vapours and the aurora borealis.
April 16. Dr. *Coste*,	Eight copies of his inaugural oration, delivered in the college of William & Mary, Virginia.
Oct. 15. Rev. *Jeremy Belknap*,	The first volume of his history of New-Hampshire.
1784, Nov. 12. Dr. *M. Guthe*,	A silver medal in memory of the Rev. Christian Meyer of Heidelburg, late a member of this Society.
——19. Chev. d' *Armours*,	A pamphlet in manuscript, on the subject of animal magnetism, by Dr. P. Hervier, of the Sorbonne.
Dec. 9. Monsr. de *Marbois*,	Report of the commissioners appointed by the king of France, to examine into the merit of Dr.

DONORS.	PRESENTS.
	Dr. Meſmer's late diſcoveries of animal magnetiſm.
1785, Jan. 22. Mr. *Mandrillon*,	An elegant copy of his work entitled le ſpectateur Americain.
Mar. 4. Mr. *W. Henry*,	The model of a wheel carriage, to run againſt the wind by the force of the wind alone.
	Two very large pieces of chryſtal found in Lancaſter county.
	An exceedingly large tuſk and one of the grinders of ſome unknown ſpecies of animals, brought from the neighbourhood of the Ohio.
Aſſembly of the State of *Pennſylvania*,	A lot of ground in the State-houſe ſquare, on which to erect a building for the accommodation of the Society.
Mrs. *Pauli*,	A horn of the Canada mooſe deer of an extraordinary ſize.
July 15. Rev. Mr. *Muhlenberg*,	A manuſcript copy of his *Flora Lancaſtrienis*.
Dr. *Noel*,	Three volumes of the proceedings of the Royal French Academy.
	An elegant electrical machine, with the neceſſary apparatus.
Sept. 27. Dr. *T. Redman*,	Specimens of the Eaſt-India manner of writing, on long ſtrips of the leaves of the cocoa nut tree.

Monſ.

1785.	DONORS.	PRESENTS.
Sept. 27.	Monſ. *C. L. d'Heritier*,	A collection of his botanical prints with their deſcription, in Latin.
Decem. 2.	Dr. *Hutton*,	His large tables of logarithms, &c.
		Three pamphlets, containing the hiſtory of the late debates in the Royal Society.
	Mr. *Sam. Vaughan*, jun.	Four pamphlets in the German language, containing new diſcoveries in chemiſtry.
	Mr. *Ed. Huſſey Delaval*,	His enquiry into the cauſe of the permanent colours of opaque bodies, 8vo.
	Arthur Young, Eſq.	His annals of agriculture.
	Mr. *Warvill*,	His tableau des ſciences & arts.
	Mr. *J. H. de Magellan*,	Botanica characteriſtica, by Dr. Wolf of Dantzick.
		A remonſtrance againſt Voltaire, by Mr. Needham.
		Lettre du Pekin, on the Chineſe language.
		Elements of mineralogy, by Richard Kerwin, Eſq.
		Medical virtues of digitalis purpurea, or fox-glove, by Dr. Withering of Mancheſter.
		Twelve copies of his pamphlet on the glaſs apparatus and eudiometer, third edition.
		Propoſals of Mr. Taylor, for printing a table of logarithms and of ſines, tangents, &c. to every ſecond of the quadrant.

Mr.

1785.	DONORS.	PRESENTS.
Sept. 27.	Mr. *J. H. de Magellan*,	Count Brahl's account of the going of Mr. Emery's new chronometer for the pocket, which ſcarce differs two ſeconds from mean time in 24 hours. Price 90 guineas, in a ſilver caſe. An account of a new hydroſtatic balance, by Mr. Nicholſon.
	Samuel Vaughan, Eſq.	Deſcriptions of uſeful machines and models of the ſociety (in London) for the encouragement of arts, manufactures and commerce, illuſtrated with copper plate deſigns, 2 vols. folio, laſt edition. Memoirs of the American academy of arts and ſciences, 4° 1785.
Dec. 16.	Mr. *Cha. W. Peale*,	An elegant portrait of Doctor Franklin.
1786, Jan. 20.	Mr. *H. Marſhall*,	His book, entitled, an alphabetical arrangement of the foreſt trees, &c. of North-America.
June.	Dr. *Ed. Waring*, by Mr. *S. Vaughan*,	Meditationes algebraicæ. Miſcellanea analytica, de æquationibus algebraicis, et curvarum proprietatibus. Meditationes analyticæ. Proprietates algebraicum curvarum.—All written by himſelf.

DONATIONS.

The following Donations were received through the hands of Dr. FRANKLIN.

1786. DONORS.	PRESENTS.
June. Monſ. the Count de *Buffon*,	His collection of birds, beautifully engraved and coloured. Supplement à l'hiſtoire naturelle, 5 tome, 4°.
Dr. *Fothergill*,	Illuſtratio ſyſtematis ſexualis Linneæ, per Johannes Miller.
Lord *Stanhope*,	Roberti Simſon, M. D. opera reliqua. 4°.
Signior *Manini*,	Le lettere Americane, 3 vols. 12°.
Monſ. *Grivel*,	L'iſle inconnue, ou memoires du Chev. des Gaſtines, 4 vols. 12°.
Mr. *J. H. de Magellan*,	His collection, de differens traités ſur des inſtruments d'aſtronomie, phiſique, &c. Extrait d'une lettre de M. Magellan ſur une pendule de ſon invention. Deſcription d'une machine nouvelle de dynamique, &c. Nouvelle conſtruction d'alambic pour faire toute ſorte de diſtillation en grande, avec le plus d'economie dans l'operation, et le plus d'avantage dans le reſultat.
M. Court de *Gebelin*,	Monde primitif comparé avec le monde moderne, 1 vol. 4°.
The Author,	Le triomphe du nouveau monde, reponſes academiques, formant

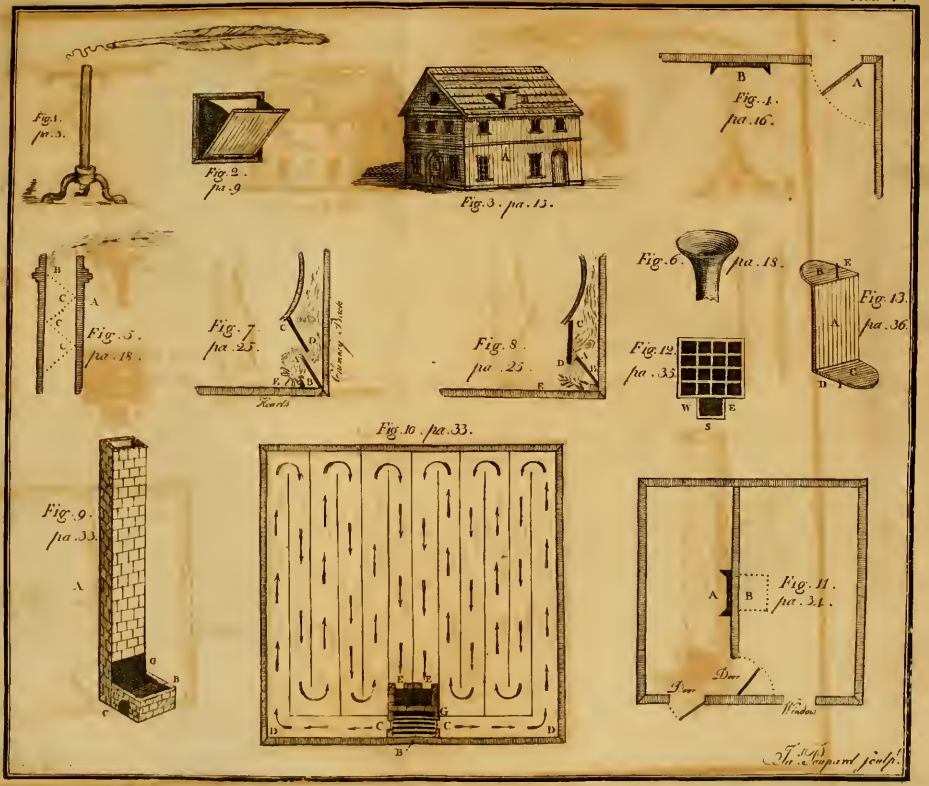
1786. Donors.	Presents.
	mant un nouveau ſyſtême de confederation, fondé ſur les beſoins actuels des nations chrétiens-commercantes, et adapté à leurs diverſes formes de gouvernement, 2 vols. 8°.
June. M. *Elie de Beaumont*,	Memoire pour Dame Marie Reine Petit de la Burthe, Marquiſe d'Anglure, contre le Sieur Pierre Petit.
Pere Berthier,	Hiſtoire de premiere temps du monde, 8vo.
Monſ. *Meſmer*,	Précis hiſtorique de faits relatifs au magnetiſme animal, 1 vol. 8vo. ſix copies.
Monſ. L'Abbé *Sans*,	Gueriſon de la paralyſie par l'electricité, 8°.
Dr. *Ingenhauſs*,	Ingen-Hauſz vermiſchte ſchriften, 1784, 2 vols. 8vo. Obſervations ſur la conſtruction et l'uſage de l'eudiometre du M. Fontana,—both written by himſelf.
Marcus Lemort Demetigny,	Tentamen ΨΥΧΟ-ΣΩΜΑΤΟ-ΙΑΤΡΙΚΟΝ ſeu conſpectus theſiformis de naturâ animæ et corporis, &c. written by himſelf.
Monſ. *Gerbier*,	Lettres et obſervations de M. Gerbier, docteur en medicine, l'un des médicins de monſieur ſervant par quartier, au ſujét de deux nouveaux remedes, contre les maladies ſquirrheuſes, cancereuſes, &c.

Pere

1786.	Donors.	Presents.
June.	*Pere Beccaria,*	Della electricita terreſtre atmoſferica a cielo ſereno, oſſervazioni di Giambatiſta Beccaria.

Sundry other books, given by different perſons, will be mentioned in the liſt to be publiſhed in the next volume.

END OF THE SECOND VOLUME.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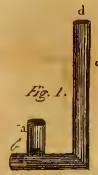
Door
Window
Ja. Toupard sculp.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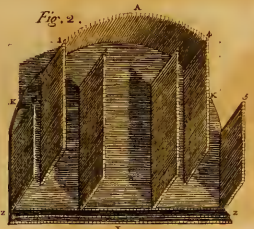
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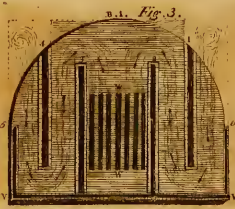
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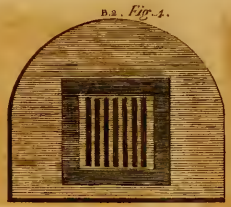
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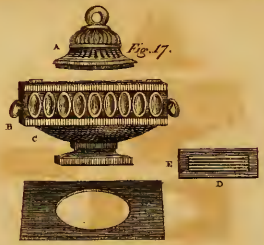
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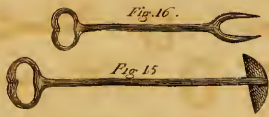
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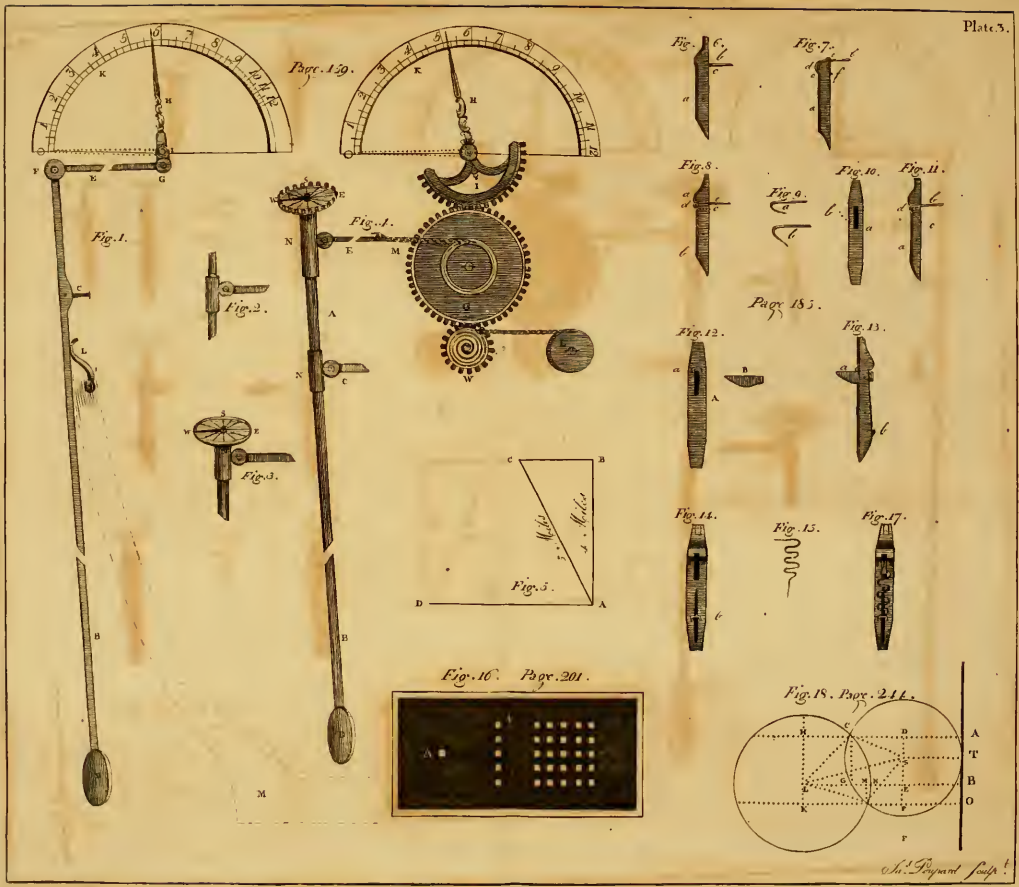
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Jn.t Poupard sculp.t

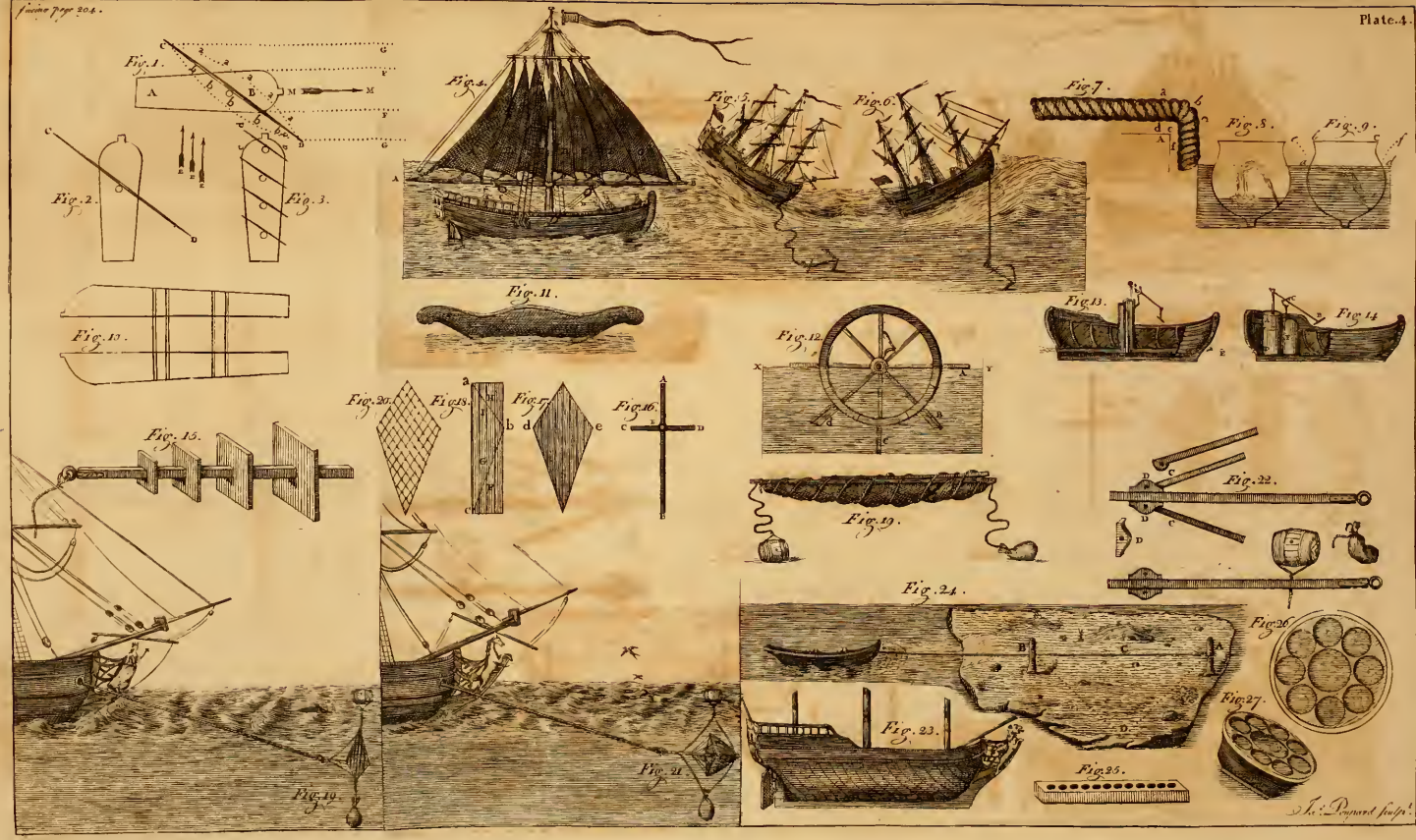
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Fig.27.
Ja: Poupart sculp.

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